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AN EXPERIMENTAL STUDY OF THE VISUAL NEGATIVE AFTER-IMAGE[1]

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Katz[3] tells us that the visual negative after-image is a film. The purpose of the present Study was to test this statement, to obtain descriptive reports of the image, to learn the effect of certain perceptive motives upon it, and to compare the images from surface and film, and from various sorts of tridimensional objects.

All work was done in a dark room. In every experiment the *O* sat 2.5 m. from the presented stimulus, and viewed it binocularly through a black tube, 1.25 m. long and 25 cm. in diam.

The *O*s in Experiment I, completed during the summer session of 1921, were Dr. L. B. Hoisington (H), assistant professor of psychology; Dr. J. Gleason (G), assistant professor of psychology at Vassar College; Dr. H. G. Bishop (Bi), instructor in psychology; Miss C. C. Braddock (Br), Miss Amen (Am), Mr. Andrews (An), and Mr. Talman (T), graduate students in psychology. Bi, Br, G, and H were trained *O*s; Am, An, and T were relatively untrained. In Experiments II, III, and IV, carried on during the following winter, the *O*s were Bi, Br, and H; and Mr. J. P. Nafe (N) and Mr. A. C. Reid (R), graduate students in psychology, comparatively untrained in observing.

In every series the *O* was instructed to fixate the stimulus and to describe, in terms as nearly attributive as possible, the after-image which it occasioned. He was asked to note especially how the texture of the after-image compared with that of the stimulus. The exposure periods for the stimuli varied from 30 to 60 sec.

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[1]From the Psychological Laboratory of Cornell University.

[2]Miss Braddock carried out all the experiments here reported; her untimely death prevented her from working up the results for publication. Dr. Grace Adams, who had followed the course of the investigation, very kindly offered to undertake this difficult and responsible task, and the paper now printed (including the Table) is due to her friendly interest.—*Editor.*

[3]D. Katz, *Die Erscheinungsweisen der Farben*, 1911, 56ff.

Experiment I. After-images from Surfaces and Films

Series i: *Surfaces.* The stimuli were round pieces of Milton Bradley paper, 8 cm. in diam. (numbered 1 red, 2 yellow, 3 green, 4 blue, 5 black, 6 white), attached to a screen of neutral gray cardboard, illuminated from both sides. The *Os* projected the after-images aroused by these stimuli (*a*) upon the lids of the closed eyes, or (*b*) upon a gray screen identical with (and drawn immediately in front of) the screen against which they had seen the stimulus. In projection (*a*) *E* turned off the lights as *O* closed his eyes.

Series ii: *Films.* The colors of the films were those of the surfaces. Our means of reduction was that which Martin describes. The openings in the neutral gray screens, 40 cm. apart, were 10 cm. in diam. The methods of projection of the after-image were the same as in Series i.

Here, as in the subsequent experiments, we left the choice of the colors to chance; but while later we completed each series within an experiment before beginning the next, here we presented first a surface color, and then the corresponding film.

Experiment II. Flat Objects

Series i. The stimuli were flat figures cut from Milton Bradley papers, mounted on cardboard, and representing twelve familiar objects (1 rabbit, 2 soldier, 3 leaf, 4 teapot, 5 cat, 6 hen, 7 flag, 8 house, 9 bottle, 10 yacht, 11 boot, 12 seascape in picture frame). The size of these objects varied from 3 by 2 to 5 by 4 in. Their colors were so chosen that the colors of the after-images should be "natural." The first six stimuli had only one color; the last six, two or three.

Series ii. The objects were the same as in Series i, but the dark figures had a narrow white border, and the light figures a narrow black border. In both series the objects were presented against a vertical gray background, and the after-images were projected by methods (*a*) and (*b*).

Experiment III. Bulky (Transparent) Objects

Series i. The seven bulky objects (1 a small bottle filled with red liquid, 2 a small bottle filled with yellow liquid, 3 a blue glass cube, 4 a green glass oval, 5 a blue glass oval, 6 a clear glass oval with colored flowers, 7 a clear glass cylinder with green stripes) stood upon a narrow glass shelf, projecting from the vertical screen. Behind them, to enhance their color, were white cardboards, cut to their shape.

Series ii. Stimuli 1, 2, 3 and 4 stood upon a horizontal ground glass screen, illuminated from below.

Series iii. Black-lined tissue paper was placed between the ground glass and the four objects of Series ii. The black lines showed through the transparent objects.

The after-images were still projected (*a*) upon the closed eyes or (*b*) upon a gray screen.

Experiment IV. Opaque Tridimensional Objects

Series i. The place of the five stimuli (1 a pasteboard cube with white outlines and faces of light and dark red, 2 the frame of a cube, made from black wood, 3 a small china cup painted yellow, 4 a red plaster apple, 5 a gray toy horse) was on the glass shelf of Experiment III. A fine thread, with a knot for fixation point, hung in front of every object. To the projection methods already described three others were added: (*c*) the object was snatched away, and the *O* was instructed to continue to fixate the knot; (*d*) the object was left visible, but the lights were dimmed so that its color could not be seen, and the *O* was instructed to project his image upon this darkened object; (*e*) the *O* was instructed to project his image on the black wall of the dark room. Methods (*a*) and (*c*) were employed for all five objects, (*e*) for only 1 and 5, and (*b*) and (*d*) for 1 alone.

Series ii. The five objects, with fixation thread in front, stood on the horizontal glass of Experiment III. We used projections (*a*), (*b*) and (*c*), as in the first series.

Results

The negative after-image, critically regarded in its own right, is always a film. This statement anticipates what is to come, and apparently contradicts many of our reports; but to avoid ambiguity, we make it here. Martin has familiarized us with the true pretextural, predimensional, unlocalized film, and its changes with localization[4]. Our after-images, while affected by localization, were subject to other perceptive motives also. Hence our *O*s gave reports which, while they in no wise contradict the Martin findings, do, owing to the different trends of the problems, vary from them. By careful analysis and after many re-cataloguings, we have been able to sum up these reports in the following table. The table includes all descriptions given throughout the entire experiment, except those of hue and chroma. These we have purposely omitted: hue, since it was determined by the stimulus; and chroma, because, although in their general remarks the *O*s spoke of the goodness of the chroma of most of the after-images, they characterised it specifically only when it was unusually and startlingly vivid, or when it was so poor as to make other description almost impossible. If then we should give the frequencies of the reports "brilliant," "good," "poor," the chromas of the images in general would seem much poorer than in fact they were. We note only that somewhere in the first experiment every *O* described chromas which were merely "good;" but that after this experiment H alone characterised chroma when it was neither "brilliant" nor very "washed out."

We did not fractionate our *O*s' reports into descriptions of Texture and Dimensionality, Localization, Shape, and Objectivity, until the experimental work was entirely over. We then fractionated them in many different ways before we decided on these four headings for our chart. At the end we found that, when we had given a report its proper place in one or more of these groups, we had, except for hue and chroma, completely analysed the report. From the general instructions it was not expected (and indeed, because of the complexity of the experience, it was impossible) that every report should mention all five characters. What is surprising is that—despite the general nature of the instructions, the complex experiences, and the disparity in the training of the *O*s—every *O* at one time or another noted every character, and gave descriptions which

[4] M. F. Martin, Film, Surface, and Bulky Colors, and their Intermediates, this JOURNAL, xxxiii, 1922, 451 ff.

exemplified every sub-group of that character; and that these descriptions are so consistent from *O* to *O* and from experiment to experiment that we can record them all in a single chart. For our problem this simplicity is fortunate and important; it allows us by mere inspection to note how slightly the images of one experiment differ from those of another. This is our significant gross result. The detailed results are the sub-groups. The rest of the paper we shall devote to exemplifying these sub-groups, and to showing their relations to one another.

No. Reports	Exp.	Series	Proj.	Localization				Texture and Dimensionality					Shape					Objectivity		
				1	2	3	4	1	2	3	4	5	1	2	3	4	5	1	2	3
105	I	i	*a*	28	9	0	0	72	14	2			22	16						
105			*b*	0	54	6	34	54	12	34			35	20						
105		ii	*a*	45	15	0	0	75	12	6			24	15	1					
105			*b*	3	54	3	25	57	10	27			17	11						
120	II	i	*a*	14	4	0	0	92	17	2	1	0	29	30	2	19	7	22	34	10
120			*b*	9	29	5	23	83	7	20	3	3	18	36	3	29	11	26	30	16
180		ii	*a*	2	2	0	0	113	34	7	12	3	15	42	1	34	20	31	45	18
180			*b*	6	14	4	27	96	1	40	3	5	18	43	1	40	26	28	49	24
70	III	i	*a*	7	2	0	0	56	8	1	1	2	10	17	1	10	1	38	16	3
70			*b*	4	4	2	8	49	4	9	1	1	16	19	2	12	1	36	20	2
20		ii	*a*	0	1	0	0	12	2	0	0	3	10	4	1	0	1	5	6	
20			*b*	0	0	0	4	19	2	1	1	2	7	10	0	4	1	10	7	0
20		iii	*a*	5	0	0	0	6	3	0	0	2	6	4	0	5	2	4	2	1
20			*b*	0	8	0	3	10	3	4	0	2	6	5	0	7	2	6	5	2
20	IV	i	*a*	0	0	0	0	24	1	1	0	2	10	16	1	11	3	15	24	2
50			*b*	0	5	0	2	4	1	1	0	2	3	9	0	1	3	4	14	0
100			*c*	0	22	13	6	37	0	13	0	20	15	26	0	31	8	25	51	2
20			*d*	0	8	0	8	3	4	0	0	0						2	7	7
20			*e*	1	4	0	0	8	0	0	0	3	2	6	0	5	0	7	11	0
20		ii	*a*	2	3	0	0	20	3	0	0	4	10	6	1	5	3	10	16	4
50			*b*	0	9	1	1	5	1	3	0	0	2	4	0	3	5	3	7	0
50			*c*	0	23	8	9	18	2	3	0	4	18	12	1	14	0	10	4	2

Key to Numbers

Localisation
1 No loc.
2 Loc. in neighborhood of screen
3 Loc. at screen
4 Loc. before screen

Texture and Dimensionality
1 Typical film
2 Thick film
3 Thin film
4 Almost surface
5 Almost bulk

Shape
1 No shape
2 Indefinite shape
3 Shape divergent from stimulus
4 Definite shape: contours blurred
5 Definite shape: contours sharp

Objectivity
1 No obj.
2 Partial obj.
3 Full obj.

Texture and Dimensionality

We treat texture and dimensionality together because the *O*s invariably reported them together; as one *O* (H) has it, "Texture and dimensionality seem bound up together." That is to say, the textureless film was dimensionless, the textured surface was bidimensional, and the tridimensional

bulk was "glassy." The surfaces and bulks, always merely "flashes" of surfaces and bulks, depending on a perceptive attitude and disappearing with any shift of this attitude, fall into only two general classes. The films, on the other hand, remaining films when critically regarded, show three more or less distinct varieties. Their differences were apparently not of a perceptive, but of an attributive kind, and the *O*s likened them to the differences in volume of tones.

(1) *Typical Film.* First, we have the typical pretextural, predimensional film which is neither thick nor thin, which 'invites' one to look into it, which is luminous and soft, and which suggests both tridimensionality and bidimensionality.

Bi (I ii *b* 1)[5] "Hints strongly at tridimensionality, yet not tridimensional; verges on surface, yet not surface. No substance in the color itself." Bi (I ii *a* 2) "After-image all of a piece with the visual field;. . . whole field lively, kinetic." H (I i *b* 1) "A bit of bright glowing color, not surface, nor solid, nor liquid, more nearly gaseous." H (I i *b* 2) "Luminous cloud effect, uniform medium that suggests depth, translucent. Light penetrates through from behind." H (I ii *a* 1) "No grain or texture to it. Like a film of translucent color; just like a self-luminous patch of color with hint of depth." Br (I i *a* 6) "Frontness vague and indefinite, yet looks softish, and suggests flatness in a way, and also depth." An (I i *a* 2) "Cloudy, with a sheen of brilliancy to it. Tridimensional, not as a body, but as a cloud." Am (I ii *b* 4) "Misty, as if you could look into it." Gl (I i *a* 6) "Soft and yet compact, not surface, not deep; . . . a sort of cloud that suggests depth." Gl (I ii *a* 3) "Think of it as having depth because it is a haze." T (I i *a* 5) "Filmy, downy, soft, cloud-like, more or less density." N (II i *b* 4) "Filmy, as if made of colored gas, and you can see into it; cloudy, as if the figure extended beyond itself." R (II i *b* 10) "Very soft, cloudy, semi-transparent." R (II i *b* 11) "Cloud-like, soft effect."

(2) *Thick Film.* Sometimes, most frequently with projection (*a*), the films, while definitely not tridimensional, were definitely thick and massy.

H (I ii *a* 3) "Had a strong suggestion of volume to it. Slightly less translucent." H (I i *a* 4) "Seems massive somehow." Bi (I i *a* 3) "Thick and bulky, simple attributive character of volume, without dimensionality." Br (I i *a* 5) "Seems more massy and solid." Am (I i *b* 4) "Appeared thick." Gl (I ii *b* 1) "Green, duller than red, and not so luminous, but seems to have more thickness." T (I i *b* 6) "Density was quite heavy." H (II ii *a* 5) "Greater amount of density to it." N (II i *a* 1) "Had considerable depth." R (II i *a* 10) "Heavy, having more body." Bi (II ii *a* 7) "Thick and bulky film. When bulky less transparent, but more massive, this one not glassy." Bi (III iii *a* 4) "Dense film, pseudo-bulk." Br (III iii *a* 2) "Thick, dense, soft, loose; . . . thick, but not bulky." R (III i *a* 5) "Heavy, more compact."

(3) *Thin Film.* Often, especially with projection (*b*), films were or became very thin. This type was rarer with closed eyes, because the *O*s had difficulty in abstracting the images from the visual field itself, which is a thicker film.

H (I ii *b* 3) "Very thin, very little body to it, like colored light on cardboard." H (I i *b* 2) "Was thin, yet a little cloudy, misty with a kind of depth and translucency." Bi (I ii *a* 4) "As yellow gets less in chroma it gets thinner." Am (I i *a* 4) "Don't think it had depth, but looked like auto-light in distance, yet suggested depth." Br (I i *b* 3) "Thin." T (I ii *a* 5) "Flat, but can imagine depth to it; . . . a trifle filmy." Br (I i *b* 2) "Thin and filmy, but not filmy in the sense of thick." Gl (I i *b* 6) "Would not suggest tridimensionality, but it is soft." H (I ii *a* 1) "Don't find that

[5] I refers to the experiment, ii to the series, 1 to the stimulus in the series, and *b* to the method of projecting the image.

when it grows thinner it seems nearer to surface." Bi (II i *b* 6) "Thin and dilute." Bi (II i *b* 12) "Soft, fuzzy, flat, which is really only the suggestion of flat." Br (II ii *b* 6) "Fairly flat, smooth, yet filmy." Br (II ii *b* 7) "Fairly thin, though loose." R (II i *b* 12) "Thin, transparent, like light thrown on screen." R (II i *b* 3) "Thin, flat, cloud-like."

The interpreting of a thick or thin film as surface or bulk was arbitrary, and depended upon its localization and the meaning of its stimulus.

(4) *Almost Surface.* Types (4) and (5) always accompanied an attitude which was more or less completely the attitude of the object-consciousness. We name them "almost surface" and "almost bulk" because, critically regarded, they never remained real surfaces or real bulks. They either appeared both surfacy and filmy, or both bulky and filmy, at the same time; or else suddenly, with growing criticalness on the part of *O*, changed from surface to film, or from bulk to film.

H (II ii *a* 1) "Seemed to be more nearly a surface; but I was in a perceptive attitude, trying to see how objective it could be." N (II ii *b* 3) "As long as it means 'maple leaf' it is a surface and flat; as it loses its outlines and gets into a mass of color, it takes on penetrableness and filminess." Br (III i *b* 1) "Soft surface, almost meant book." H (II ii *b* 4) "For the moment it seemed opaque, dull, flat." N (II i *b* 2) "As it fades it loses meaning and becomes less surfacy."

(5) *Almost Bulk.* H (II ii *b* 7) "At first very suggestive of a solid mass; somewhat glassy, and seemed to have real tridimensionality." H (II ii *b* 9) "Took on some characters of bulkiness; seemed more glassy and transparent." R (II ii *b* 9) "Tendency to glassiness." R (II ii *b* 8) "Green at first like a shadow; then when it meant 'house' was solider." H (II i *b* 2) "When detached from the screen a little harder, glassier, a little less internally active and slightly more translucent. Suggested depth, though still not so soft and loose. Has a tendency toward bulk." R (III i *b* 2) "When it becomes objective, it becomes more glassy and a little transparent." N (III ii *b* 3) "Difficult to say if there is a difference in texture when it becomes a cube. Have an impression of seeing through it; yet it is different from looking through glass." Bi (IV i *c* 1) "Hint of glassy." Br (IV i *c* 1) "Foggy and filmy, but more transparent. Don't know whether really a thinner film or actually of a more glassy and harder texture." H (IV i *c* 1) "Hint of bulkiness, glassiness." H (IV i *a* 1) "Fleeting impression of dense, bulky, glassy thing, floating."

Localization

(1) *No Localization.* As a film is predimensional and pretextural, so it has no position. With projections (*a*) and (*c*) indefinite localization was the common report; even with projection (*b*) it was not infrequent.

Bi (I i *a* 4) "Localization very uncertain." H (I ii *a* 1) "Localized out somewhere." H (I i *a* 4) "Not definitely localized." Gl (I i *a* 4) "Can't place it." Gl (I ii *b* 1) "Might have been anywhere." An (I i *a* 1) "Suspended off in space." Am (I ii *a* 1) "Seemed sunk in the dark." Bi (I i *a* 5) "Localization indefinite, near or far wherever I fixate it." Bi (II ii *a* 4) "On eye-lids colors are as indefinitely localized as the sky is." Br (II i *a* 4) "Out there." N (II i *b* 2) "Not localized." Bi (II i *b* 9) "No relation to screen; screen not seen." Bi (III i *a* 3) "Nowhere."

(2) *Localization in Neighborhood of Screen.* Because the *O* has seen the stimulus against the screen out in front, he sometimes, even with closed eyes, places the image with reference to this screen. With projection (*b*) this position is customary. In neither case can the *O* state the exact relation of image to screen; it 'was near' it or 'referred' to it.

H (I i *a* 2) "Localized out somewhere in front, somewhere near place of stimulus." H (I ii *a* 2) "Localized out in front at about the region of the screen, and then became more indefinite." T (I i *a* 4) "Localized about the same place as the stimulus." Br (I i *a* 3) "Located about the same distance as the screen." H (I i *b* 2) "At best definitely not lying on the screen, but in the neighborhood of the screen, with a tendency to go beyond and through it." Bi (I ii *b* 2) "Spot was referred to the cardboard, but not really there." Br (I i *b* 3) "Localization indefinite, but it hovered near the cardboard." T (I i *b* 4) "Localization was uncertain, but I think it was back on the screen." An (I i *b* 3) "Location appeared to be about where the object is on the screen." Gl (I ii *b* 4) "Located on screen or between me and screen." N (II i *b* 11) "Near the screen, yet really does not belong to it." R (II i *b* 4) "Within or further away than screen." Br (IV i *d* 1) "It is most difficult to say whether it is really out there in the air or not. It tends to be identified with the screen or near the screen; yet when I realize I am still fixating the point, I see it there and feel that it is transparent."

(3) *Localization at Screen.* The very thin films (Texture and Dimensionality 3) could actually lie on the screen (*b*) or darkened object (*d*) upon which the *O* projected them. As films they had no texture of their own; so that in many cases they seemed merely to lend their color to the cardboard, affecting it in no other way.

Bi (I ii *b* 4) "Appeared enmeshed in grain of screen as if it belonged to it and the screen had been tinted that way there." H (I ii *b* 4) "Like a pencil of light hitting the cardboard as if it washed over it." Br (I i *b* 2) "Tinted the gray screen; seemed that it might be just a bluish part of the cardboard." An (I i *b* 3) "Located on front screen. Appears as if projected on the screen, as if an integral part of the coloring of the screen." T (I i *b* 1) "Localized on the cardboard as a part of it." Gl (I i *b* 2) "Localized on the screen, not filmy, but like a part of the screen." Bi (II ii *b* 4) "Alters the gray screen in color and tint. Has no texture of its own, but takes on that of the screen."

Even, here, if the *O* shifted his attitude ever so slightly, he noticed that the background itself was soft. With projection (*d*), because the object was so weakly illuminated that it did not present absolute surface, this softness did not detract from the reality of the object.

H (I ii *b* 4) "Makes a difference to the screen in some way, a little dulling, obscuring, softening of the surface; it is not so distinctly seen." H (I i *b* 1) "The image steals more from the screen than the screen does from the image. The gray loses its surface-character. The image got thinner, but was still slightly lively."

(4) *Localization before Screen.* At times with projection (*b*) and always, if instructions were followed, with projection (*c*), the image stood distinctly in front of the background. Here we find an approximation to bulk, but not a true bulk,—because the image did not appear "glassy," and the tridimensionality showed between the color and the screen, not in the color itself.

Bi (I i *b* 3) "Located this side of gray." Gl (I i *b* 6) "Located this side of the screen." Am (I i *b* 4) "Located in front of the screen." R (II ii *b* 7) "This side of the screen." Br (II i *b* 7) "Out in front of the screen." H (II ii *b* 7) "Out in front." N (IV i *c* 1) "Stood out on shelf." B (IV ii *c* 1) "Stands out in front." Br (IV i *c* 1) "At first clearly out on shelf."

Shape[6]

(1) *No Shape.* The film which is without texture, dimensionality and localization, is also in its own right without shape. Many of the reports bear out this fact.

H (II i *b* 1) "Just a mass." Bi (II ii *b* 5) "Round, irregular shape." Am (I ii *a* 2) "Very little definite outline." An (I i *b* 4) "Became indefinite in form." Bi (I i *b* 3) "No sharp outline." N (II i *b* 5) "Irregular shaped light spot." Br (II i *a* 3) "Soft and fuzzy outline and shape indefinite." H (III i *b* 4) "Blur of purple." R (III i *b* 3) "Just a blur."

(2) *Indefinite Shape.* We purposely saw to it, by the forms of the stimuli, that some of our after-images should have contours. Frequently, though, when the *O*'s attitude was not highly perceptive, these contours were blurred and showed no detail. In Experiment I, detail was of course impossible.

Bi (I i *b* 1) "Outline indefinite, but shape circular." An (I i *b* 4) "Circular in shape, but indefinite." Gl (I i *a* 5) "Fuzzy in outline." H (II ii *a* 2) "Poor outlines, very fuzzy, not very good reproduction of details." Br (II ii *b* 2) "Outlines good, though the details of the picture are lacking." N (II i *b* 4) "A dark square." R (II ii *b* 12) "Fuzzy, no detail." Bi (II i *b* 3) "All the big points." Bi (II *b* 8) "Windows and doors, daubs of yellow, elliptical, no corners." R (II ii *a* 6) "Crescent shape with ragged outlines."

(3) *Shape Divergent from Stimulus.* Occasionally, despite our control, the shape of the image differed radically from that of its stimulus.

N (II ii *b* 4) "Looked more like a duck." H (II i *a* 3) "Rather an oak-leaf than a maple-leaf." H (II ii *a* 4) "Not quite so sharp in outline, and distorted in shape." Bi (II i *a* 4) "Shape roundish like a little camel."

(4) *Definite Shape*: *Contours Blurred.* Even with a more or less complete object-consciousness, if the details of a figure were clear, its boundaries were almost invariably obscured, and its outlines ragged.

Br (II i *b* 2) "Outlines fuzzy, but shape clear." Br (II *a* 12) "Very, very clear, but fuzzy." H (II i *b* 4) "Outlines good, though not as sharp as in the stimulus, in form accurate reproduction of stimulus." Br (III i *a* 1) "General bottle-shape, but fuzzy edges." Bi (II i *a* 8) "Outlines sharp, but shadowy at edges, rounding it off. The after-image never looks like an object, because it is rounded off."

(5) *Definite Shape: Contours Sharp.* Seldom, and always under a perceptive attitude, these boundaries for an instant appeared clear and then, as the image faded, became ragged.

Br (II i *b* 3) "Shape very clear, outlines at first distinct, then became more and more indistinct." R (II i *b* 8) "At first rounded and fuzzy, and then lost this and became smooth; edges became smooth and whole thing more flat. Then became fuzzy again." N (III i *b* 3) "Lines at sides were well defined, got less well defined."

Objectivity

Experiment I offers no reports of objectivity; subsequently, because of the object-character of the stimuli, these reports became customary.

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[6]The observations of this paragraph are in conflict with such results as those of W. McDougall, Some New Observations in Support of Thomas Young's Theory of Light- and Color-Vision, *Mind*, N.S. 10, 1901, 52 ff. Perhaps a difference in the set or attitude of the *O*s may be responsible; it seems, as we point out later in this paper, that the after-image is subject to contamination by other modes of imagery. [Cf. W. Fuchs, Experimentelle Untersuchungen über die Aenderung von Farben unter dem Einfluss von Gestalten ("Angleichungserscheinungen"), *Zts. f. Psych.*, 92, 1923, 249 ff.,—an article which we received after the above Note was written.]

(1) *No Objectivity.* Throughout the last three experiments when an image, though it might have some position or shape, was still taken more or less observationally, we get reports of "no object-reference."

H (II i *b* 12) "No object-reference." Bi (II i *b* 12) "No objectivity." R (IV i *c* 1) "Like block of air, no texture at all, no framework, an indefinite substance." N (III i *b* 3) "No objectivity, light and dark." Br (III i *a* 7) "Dark streak, pale streak, no objectivity." N (II i *a* 4) "Took up a non-perceptive attitude." H (II i *b* 8) "Does not mean house, but then I was not in attitude for meaning."

(2) *Partial Objectivity.* Partial objectivity enters into all descriptions of surfaces and bulks which were not wholly surfaces and bulks, or which would not remain surfaces and bulks, and into the infrequent reports of sharp outlines that soon blurred. This incomplete objectivity is obviously a part of that attitude, or is one of those attitudes, which Martin did not catch and Adams[7] could not classify. We can do no better. Our partial objectivity usually manifested itself as an incompatibility between true objectivity and the filmy nature of the image. The *O*s tried to lessen the conflict or to reconcile the incompatibles by such phrases as "ghost of an object," "shadow of an object," and "object, but never a perception," but in the end had to admit that the after-image had a "personality of its own."

Bi (II ii *b* 6) "No objectivity, except that it is a little like a shadow of an object, except that it has a personality of its own." Bi (II i *b* 6) "Like the shadowy extent of an object, but too filmy to be taken for an object." H (II ii *a* 1) "It represents an object as a shadow represents an object. It seems to stand out from the background and be more nearly surface, but I was in a perceptive attitude to see how objective it could be." H (II ii *a* 7) "Conflict of apparent objectivity as against the softness and filminess of the color." H (II ii *a* 8) "Indefinite localization, filmy thing, nothing beyond it, seems incompatible with objectivity." R (II i *b* 8) "When all the colors assumed their proper place it meant 'flag.' At first it appeared rounded and fuzzy, and then lost this and became smooth; the edges became sharp, and whole thing more flat." Br (II ii *b* 3) "I look at the black line and the perceptive motive comes in, then I look into the middle and see that it is a film." N (II ii *b* 3) "As long as it means 'maple-leaf' it is a surface and flat; as it loses its outlines and gets into a mass of color it takes on penetrableness and filminess." H (II ii *b* 4) "Objectivity, but never the perception of a kettle, always image of kettle." H (II ii *b* 10) "Almost carried object-meaning, though still filmy, hazy and not tridimensional." Bi (III i *a* 1) "No more a bottle than the ghost of a man is a man." Bi (III iii *b* 2) "Like the shadow of a bottle; meaning of bottle is not the same as seeing a bottle." Br (III i *a* 2) "The objectivity of a silhouetted object." R (III i *b* 1) "Think of bottle and it seems tridimensional, otherwise flat." Br (III iii *b* 3) "Was filmy and then flashed into something cube-like. When I looked for it, it disappeared." H (III i *b* 4) "Two attitudes kept fluctuating, perceptive and descriptive; when perceptive, carried meaning of having tridimensional aspect; when as before, a fuzzy, filmy thing." N (IV ii *a* 3) "Unorganized, flat mass of yellow; then suddenly snaps into a cube-shape and becomes tridimensional and an object; it stands right up in front." R (IV ii *b* 5) "Flash of objectivity; seems to stand out at right angles; then objectivity becomes poor, means 'horse' somewhat, but only trace of tridimensionality in it."

(3) *Full Objectivity.* An after-image was a satisfactory 'object' only when the *O* 'forgot' to become critical towards its substance and outlines, or when it represented an object which could, under certain conditions (*e.g.*, lowered illumination), have ragged outlines and filmy texture.

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[7]G. K. Adams, An Experimental Study of Memory Color and Related Phenomena, this JOURNAL, xxxiv, 1923, 359ff.

Bi (II ii *a* 10) "I let the perceptive motive have its way, and saw it as a ship on the sea." N (II ii *b* 8) "A house with lighted windows and doors. The object was so good that I forgot to look at the texture." H (II ii *a* 7) "Meant British flag against sky." H (IV i *d* 1) "A very good cube, the most complete cube, and solid." Br (IV i *d* 1) "Slipped inside and was a green cube." R (IV i *d* 1) "Dark cube with white frame, same as if actual cube was there." Br (II i *a* 12) "Like a picture seen in the twilight, with the fuzziness and softness of twilight."

Two questions remain. (1) Are the two variants of film, the thick and the thin, due simply to the mode of projection of the after-image, or does the 'density' of the film vary intrinsically, *e.g.*, with hue? There is no doubt that mode of projection has its effect (see p. 161); and sporadic cases not referable to mode of projection may result from special conditions. We prefer, nevertheless, to leave the question open.[8] (2) What are the 'almost' surfaces and bulks and the sharp outlines which quickly disappear? Do they represent intrinsic changes of the film, or are they perceptive additions? We reply that they fulfill perfectly the three conditions of the imaginal overlay; and we have evidence that overlays sometimes formed part of the original experiences. For example:

Br (II i *a* 12) "A flash of green hills, but this went out and left only the yellow rectangle." Br (II i *a* 12) "The sky seemed streaked with clouds."

Conclusions

Katz' claim for the negative visual after-image is just. No matter what its stimulus, it is, when critically regarded, a film,—textureless, dimensionless, in a strict sense shapeless, and without locality. Under the conditions of our experiments, it shows differences in 'density.' If the *O*'s attitude tends toward objectivity, other, perceptive, differences appear; the images are localized; they have, for an instant, sharp outlines and definite texture. As soon as this 'objective' attitude becomes ever so slightly critical, the localization ceases to be definite, the outlines are blurred, and the image is again essentially soft and insubstantial.

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[8] As there are preformal differences that can be communicated only in terms that suggest form (K. Goldstein u. A. Gelb, Psychologische Analysen hirnpathologischer Fälle auf Grund von Untersuchungen Hirnverletzter, *Zts. f. d. g. Neurol u. Psychiatr.*, 41, 1918, 73), so may there be pretextural differences that can be characterised only in terms that suggest texture.

THE RÔLE OF KINAESTHESIS IN MEANING[1]

By Myra Shimberg

CONTENTS

I. Historical Introduction 167
II. Experimental Procedure 171
A Problem 172
B Material 172
C Observers 172
D Method 172
III. Results 173
A Reaction Time 173
B Statistical Analysis 173
C Introspective Analysis 176
IV. Supplementary Experiment 179
A Introduction 179
B Material 179
C Procedure 179
D Results 179
V. Conclusions (with Summary) 181
VI. Appendix 183
A Examples of Material 183
B Tables 184

I. Historical Introduction

The rôle of imagery in the higher mental processes has long been a matter of contention among psychologists. Marbe's experiments in 1901 and Bühler's investigations in 1907 provided the impetus for further experimental work in this direction.[2] Marbe conducted experiments in an attempt to ascertain the conscious processes involved in judgment.[3] Bühler's investigations were entitled "Ueber Gedanken". He gave his subjects certain questions to answer and insured their thinking by asking questions that could not be answered without reflection. Then he asked for full introspections.[4] He concluded that "the most essential part of the experience is something which in content is different from anything that can, as a last resort, convey its impression in sensations; . . . as the essential components of our thought experiences, *thoughts alone* can be considered."[5]

Among American psychologists, the work of Woodworth is most clearly allied to that of the Würzburg school. Woodworth set some problem which

[1]From the Psychological Laboratory of Wellesley College. Communicated by C. A. Ruckmick.

[2]From the theoretical side, Mach, Ehrenfels, Meinong, Watt, Ach and Messer in Germany, and Spencer and James in England, had already attacked this problem.

[3]E. B. Titchener, *Experimental Psychology of the Thought Processes*, 1909, 81.

[4]K. Bühler, Tatsachen und Probleme zu einer Psychologie der Denkvorgänge: 1. Ueber Gedanken, *Archiv f. d. ges. Psychol.*, 9, 1907, 312.

[5]*Ibid.*, 315, 317.

[6]R. S. Woodworth, Imageless Thought, *J. Philos. Psychol.*, 3, 1906, 703 ff.

the subject was to solve promptly; and as soon as the solution was reached, or even before, he interrupted the course of the subject's thought and called for a description of the process of seeking and finding a solution. Introspection was made more reliable by calling for answers to very definite questions as "any visual picture?" "any words heard?" "any feelings of bodily movement?"[6] An example from Woodworth's voluminous reports follows: To the question "What substances are more costly than gold?" the answer came promptly "Diamonds." The subject reported that she "had no visual image of the diamond; the thought of diamonds was there before the sound of the word. . . . It is the same way in conversation; you know what you want to say, but the words come so quickly that you don't have a chance to think before you say them."[7]

Woodworth's own introspections also revealed examples of imageless thought. One of the many instances he quotes occurred while "reading a psychological book in a foreign language." In a certain sentence occurred an unfamiliar word. On rereading it he saw the meaning from the context, "yet no English or other equivalent suggested itself until distinctly later . . . It seems impossible to describe these facts without admitting the existence of other than sensorial contents of consciousness." Woodworth believes that, in addition to "sensorial elements, thought contains elements which are wholly irreducible to sensory terms."[8]

The "imageless thought" doctrine has been effectively challenged by many psychologists. Colvin vigorously opposed it: "There is . . . . a type of motor imagery which has up to date passed unnoticed. It is a type which expresses itself in more general motor attitudes and includes all sorts of kinaesthetic images that suggest the individual's relation to a situation and which are the mind stuff of meaning. . . *Thinking is not imageless*, but does not have the rich ideation of concrete visual or auditory imagery."[9] In his introspection of motor ideation, Colvin found himself using imagery that may be termed mimetic. It consisted of *motor* images and sensations that were symbols of the situation which they represented just as truly as words represent concrete reality. Whenever he introspected his *meaningful* thinking, he invariably found this tendency.[10] "May it not be, that those who believe that they have imageless thought possess this mimetic imagery but in so slight a degree that it has never been analyzed out in their consciousness?"[11]

Titchener also stands opposed to the doctrine of imageless thought. Referring to Bühler's thesis—which would also be Woodworth's—that there are thoughts without the least trace of any sort of imaginal groundwork, Titchener criticizes Bühler's experiments as leading the subjects into the pitfalls of the stimulus-error: "Bühler's observers were men of wide psychological experience, of long technical training, of undisputed ability; but they were given an immensely difficult task, in terms of a very poor method. . . . I submit that the observer was not describing his thought, but reporting what his thought was about; not photographing consciousness, but formulating the reference of consciousness to things: in a word, that he had fallen, in the case of thought, into the error which we should term the stimulus error in the case of sensation."[12]

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[7]*Op cit.*, 703 ff.

[8]*Ibid.*, 706.

[9]S. S. Colvin, Method of Determining Ideational Types, *Psychol. Bull.*, 6, 1909, 236.

[10]S. S. Colvin, *Psychol. Bull.*, 7, 1910, 58.

[11]*Ibid.*, 59.

[12]E. B. Titchener, *op. cit.*, 146.

Recently Moore[13] has summarized the investigations concerning the place of the sensation and image in perception, memory, and thought. He points out the inadequacy of the doctrine that meaning is equivalent to some form of sensory or imaginal content, but does not feel inclined to regard meaning as extrapsychological. He lays down his own thesis in the words: "Meaning itself is a mental structure, the process by which it is obtained is a function."[14] In the course of this exposition he discusses his version of "mental structure" and "mental function."

It is not unlikely that the discussion of imageless *vs.* imaged thought has significance in connection with the question of imagery aroused with other higher mental processes. There has been considerable investigation of the rôle of imagery in voluntary movement. But recently the function of the motor processes, looming so large in the behavioristic type of psychology, has also received attention at the hands of the introspectionists. The motor attitudes connected with the cognitive and conative processes have a full share of space in our present literature. More especially the part played by the kinaesthetic sensations is being examined in its bearing on the problem of meaning and in the formation of meaningful perceptions.

The rôle of kinaesthesis in psychological phenomena has been the subject of many experimental papers. Woodworth first investigated the cause of voluntary movements. "What is the cause of a voluntary movement?" is his starting point. "A purely schematic psychology" would answer that "voluntary movement" is "movement foreseen and intended. . . There must, therefore, be in the mind an idea of the movement, and. . . . this idea will consist of reproduced sensations of sensations originally produced by the movement. . . . Therefore, the cue of a voluntary movement consists of a sensorial image of the movement." "The kinaesthetic image is the distinguishing mark, the determinant, of the coming movement." The experimental observations of Woodworth, however, led him to "deny that any form of sensorial image of the movement or of its outcome need be present in consciousness in the moment just preceding the innervation."[15]

Woodworth's experiments required the subject "to make a given movement with some preliminary hesitation and to note the condition of mind that preceded the movement" with particular emphasis on what imagery appeared, *etc.* Subjects of considerable psychological training were chosen. Results, of course, showed great individual differences in the sort or amount of imagery experienced. Out of many reports, I quote the following: "Imagery of any kind scarcely occurred at all except as the result of effort to get it. Kinaesthetic imagery so got was not like the sensations of the actual movement." In a rough summary of his results, Woodworth estimates that some sort of imagery occurred in 55% of the total cases, *one-half thus showing no imagery.*[16]

Ruckmick's experiment on the Rôle of Kinaesthesis in the Perception of Rhythm investigated the contents of consciousness during the period of the presentation of rhythm. The general conclusion reached is that "whatever the material presented for rhythmization. . . . kinaesthesis was essential for the establishment of a rhythmical perception. That perception once established, however, rhythm might be consciously carried, in the absence of any sort of kinaesthesis, by auditory or visual processes."[17]

[13]T. V. Moore, Image and Meaning in Memory and Perception, *Psychol. Monog.*, 27, 1919, no. 119.

[14]*Ibid.*, 186.

[15]R. S. Woodworth, Cause of a Voluntary Movement, *Studies in Philos. and Psychol.* (Garman Commemorative Vol.), 1906, 351ff.

[16]*Ibid.*, 361.

[17]C. A. Ruckmick, Rôle of Kinaesthesis in the Perception of Rhythm, this JOURNAL, 24, 1913, 359.

Of the experiments on *meaning* and *kinaesthesis*, those of Jacobson, Ogden and Rogers seem most significant. In reply to a criticism that Cornell observers have been predisposed against the "discovery of meanings in experience and have therefore confined their introspections to the 'known mental categories of sensation, image, and feeling in which [they] have been schooled'," Jacobson submitted a series of experiments on the meaning and understanding of letters, words, and sentences. He concluded that meaning is carried by visual and kinaesthetic processes. "So far from having a predisposition against meanings," said he, "we have . . . made a systematic attempt to cultivate reports about them. . . . We find that *wherever there is meaning there are also processes*, and that the correlated meanings and processes are *two renderings, from different points of view, of one and the same experience.*"[18]

In contradistinction to this experiment we have the investigation of Ogden on the consciousness of meaning. He presented each of his subjects with four series of words, the first containing concrete words, the second abstract, the third abstract and concrete, and the fourth nouns containing thirteen different classes of objects. He reported that in some of his series, especially the last, *imagery was a negligible factor.*[19] "Summarizing briefly, we may say that the instruction leads to the formation of a mental state upon the presentation of the stimulus word. This mental state, the nucleus, is the primary dispositional reaction to the word. It may evidence itself in consciousness as a generalized meaning, as more precise meaning contents of an imageless nature, or as a mere attitude of familiarity. It may also be quite unconscious, so far as introspection shows, and yet evidence itself in its control over the associated imagery and notions of meaning that subsequently appear. It is this relational control which constitutes the meaning of the word."[20] Ogden concludes by saying that, because of the complexity and variability of his stimuli, the experiences "were difficult to analyze. . . . A similar investigation under conditions of brief exposure with words or pictured objects would perhaps be conducive to a more precise analysis."[21]

Such an investigation was started, at any rate, in Rogers' Analytic Study of Visual Perceptions. Rogers used as stimuli twelve series of ten irregular inkblots presented visually. After the exposure period the *Os* were asked for a full introspective report of all mental processes occurring between the beginning of the exposure and the distraction-stimulus.[22] The following results are pertinent to our discussion. (1) Kinaesthetic sensations represented the largest percentage (36) of any particular class of mental phenomena reported. (2) All imaginal materials considered to-

[18]E. Jacobson, Meaning and Understanding of Letters, etc., *ibid.*, 22, 1911, 574-575.

[19]R. M. Ogden, Some Experiments on the Consciousness of Meaning, *Titchener Commemorative Vol.*, 1917, 80ff.

[20]*Op. cit.*, 89f.

[21]*Ibid.*, 120.

[22]A. S. Rogers, Analytic Study of Visual Perceptions, this JOURNAL, 28, 1917, 538. V. also: Auditory and Tactual Perception: the Rôle of the Image, *ibid.*, 34, 1923, 250ff. Some of her results are directly comparable to those we have just discussed. She states (258) that "the more complex the stimulus, the greater is the tendency to reproduce the direct sensation in imagery." On the other hand (263), "the images which served to elaborate the meaning of the perception lie without exception for the greater part in the background of attention." On the whole "these simple images render very definite services to the auditory and tactual perceptions; namely, those of reproduction, interpretation, orientation and elaboration" (265).

gether formed about one third (35%) of all processes, of which visual (27) and kinaesthetic (7) were most predominant.[23] (3) The kinaesthetic sensations and the auditory verbal images performed the function either of *bearing meaning* or of self-instruction *to seek meaning* in the figure.[24] In conclusion Rogers states that the results agree in general "with the view maintained by several writers—James, Bagley, Wallaschek, Lehmann, and Titchener—that the background, sensational or imaginal, is of fundamental importance to the meaning of perception. The kinaesthetic and organic sensations and the imaginal processes under our conditions have consistently formed a setting for the central visual complex and have supplied more of the derived significance than the 'directly' initiated processes themselves."[25]

Thus Rogers, contending for an intimate relation between meaning and process, is at variance with Ogden, already cited, who concluded that the meaning of a word is given in a "mental act of relating."

II. Experimental Procedure

This experiment on *meaning* was not intended to add material to either side of the discussion on imageless thought, though indirectly the subjects are inevitably linked. The special problem of the paper, as indicated in the title, is to ascertain the part that *kinaesthesis* plays in the search for *meaning*. One of Moore's criticisms, if valid, would have some bearing on our study. He claims that there are certain disadvantages in the use of nonsense material for the solution of questions regarding meaning. "Recall—especially the recall of nonsense syllables—is a mechanical something, and, though it does involve the higher processes, the main factor that comes out is imagery more or less obscure to which one can not attach any meaning whatsoever."[26] While we should be inclined to disagree with him in the extreme position of the latter statement, we recognized the desirability of selecting material which should be progressively more meaningful, in order to get a genetic survey of the meaning process. In our experience we have found very few *O*s who could not "attach any meaning whatsoever" to the most forbidding nonsense syllable. But we selected our material in such a way that the syllables would become progressively more complex and therefore furnish additional anchorage for meaning. The investigation followed an earlier experiment which attempted to ascertain the ease of learning Esperanto as compared with that of Danish words. The results were distinctly in favor of Esperanto. This was undoubtedly due to the fact that Esperanto abounds in words of Romance origin which were not entirely unfamiliar to the *O*s. Esperanto words were, therefore, selected as the medium in this experiment, because it seemed that they would

[23] *Op. cit.*, 540.
[24] *Ibid.*, 546.
[25] *Ibid.*, 575.
[26] *Op. cit.*, 277.

supply the link between the Ogden and Rogers experiments, and would also meet the criticism of Moore: *i.e.*, they would be unfamiliar, and yet not entirely devoid of possible intrinsic significance.

A. Problem. In the first part of this experiment, the object was to ascertain how the meaning of certain Esperanto words was carried.

B. Material. For this purpose five series of words were used. These were constructed as follows.[27]

(1) The first series (I) consisted of 20 words[28] of three syllables each. All these words were nouns. This insured the Esperanto noun-ending in "o" and the Esperanto antepenultimate accentuation, thus giving a certain uniformity and rhythm to the list. *E*, moreover, carefully excluded (*a*) all words containing a harsh combination of vowels and consonants, and (*b*) all words which had any obvious affinity to English or French words, as, for example, *agrabla* = agreeable; *aceter* = buy.

(2) Series II was intended to be more difficult, and so longer, more nonsensical-sounding words were chosen. These were all of four syllables, and were chosen in the manner described above, except that a few verbs were added so that the series was not as uniform as the first.

(3) Series III consisted of words of 2 syllables. In an attempt to make this series still more difficult, noun-stems, adjective-stems, and verbs were inserted, thus securing dissimilar endings. We found that the disyllabic words in this series presented more difficulty than words of 3 or 4 syllables in the former lists. As each short word was presented *O* was forced to take it as a whole, whereas the longer words offered the opportunity of picking out a syllable already meaningful.

(4) Series IV was easier than any of the series heretofore presented. It contained trisyllabic words, each selected with regard to its approximation to some English or equally familiar word.

(5) Series V contained short, easy words, characterized by their close approximation to familiar words. The rhythm was carefully noted, all the words being (*a*) nouns, thus ending in "o"; (*b*) of two syllables; (*c*) accented on the first syllable; and (*d*) smooth and pleasant to the ear. It is obvious, from the preceding enumeration of precautions taken, that this list was intended to be the very easiest of all.

C. Observers. The *O*s who participated in this experiment were all connected with the Department of Psychology. They were Dr. E. A. McC. Gamble (G),[29] Professor of Psychology; Miss A. Shuey (S) and Miss M. Amig (A), Graduate Assistants; and Miss D. Macomber (M), an undergraduate student in the Department.

D. Method. The *O*s worked individually with *E* in a room kept as free from distraction as possible. Reactions were recorded by requiring the *O*s to press down a finger reaction key, and the time was measured in σ by a Dunlap chronoscope in conjunction with a stop-watch, which aided in checking up the longer intervals.

The *instructions*, adapted from those of Ogden, were: "Close your eyes and hold yourself as passive as possible, both mentally and physically. After saying 'ready', 'now', I shall pronounce a word. Give it your immediate, full attention with the definite purpose of understanding the meaning

[27]For specimen lists see *Appendix*.

[28]The number of words in the different series varied as *E* discovered *O*'s learning capacity in the allotted time.

[29]G is a highly trained *O*. She has attained considerable facility in learning nonsense syllables, with which she has been experimenting for many years.

it assumes. As soon as you are satisfied that the meaning has been grasped, react by pressing the key in front of you. Be in no hurry to react, and let your occupation with the word be as natural as possible."[30] Then *E* pronounced each Esperanto word in a quiet monotone. No hint of the English meaning of the Esperanto word was given, and care was taken that *O* should not have had previous acquaintance with it. Furthermore, no definition of *meaning* was provided. One *O* (G) demanded a further explanation, and was finally told to react when the associations, which arose, should lend their meaning to the word. None of the other *O*s raised any question, and each was allowed to solve the problem in her own way.

III. Results

A. Reaction Time. The average reaction time (in sec.) of each *O* in all five series is recorded in the following Table. The series are given in order of increasing difficulty. (Series I of G and M and Series II of M are omitted because of inaccurate timing on those occasions.)

Series	A	G	M	S
V	2.57	2.23	2.26	1.05
IV	3.01	3.13	3.09	1.05
I	9.50	——	——	1.76
II	5.96	5.90	——	.90
III	4.54	3.52	3.18	.85

A on the whole took the longest time to react. This is perhaps explained by the fact that it was one of her first attempts at introspection and that she was extremely anxious to do just as she was told.

The type of word seemed to have some effect on the reaction time. For A, G and M the reaction time decreased from Series III to V. They thus found less difficulty in reacting to the simpler, shorter words, especially when uniformly presented. The variation of S may be due to the fact that in Series II and III she almost invariably reacted to the first syllable of the word instead of to the whole, thus securing a shorter reaction time in those two series than the other *O*s.

We shall refer in more detail to these reaction time results in the analysis of content.

B. Statistical Analysis of Content. The appended Tables are intended to embody a summary of the processes occurring between the stimulus and the reaction as reported by *O*.[31] An explanation and analysis follows.

(i) In Table I the contents, reported in all five series of the experiment, are given in percentage values for each *O*. It is obvious from this Table that for every *O* kinaesthesis, especially in the form of internal speech, played a very important part.

[30] Cf. Ogden, *op. cit.*, 79.

[31] Form partially adapted from Ogden, *op. cit.*, 84.

Kinaesthesis, in fact, represented 57.06% of all images reported. Kinaesthetic and visual imagery together formed at least 85% of every *O*'s total imagery. For G and M visual imagery was predominant; for A and S kinaesthesis seemed to play the most important rôle.

Percent. Total Images Reported

	Kin.	*Vis.*
A	68.33	15.83
G	39.89	60.7
M	30.92	58.76
S	84.23	11.61

The approximate distribution of kinaesthesis in the different series, given in increasing order of difficulty, was as follows.

Series	A	G	M	S
V	59	24	17	82
IV	58	39	29	83
I	78	39	53	82
II	66	53	34	77
III	87	51	39	91

The *O*s do not agree at every point, but some significant generalizations can be made. With one exception (M) Series II or III or both seemed to evoke most kinaesthesis. *Without exception* Series III contained a larger percentage of kinaesthesis than Series V. Since the former was the most difficult, and the latter undoubtedly the easiest, it seems obvious that kinaesthesis must play its most important part in the striving after an elusive, difficult meaning.

It may be noted, moreover, that the *O*s who reported the greatest amount of kinaesthetic imagery agree in practically every respect: the peak of kinaesthetic imagery is reached in III, a decided waning occurs in IV and V.

(ii) Table II was compiled in the following way. The percentages were determined by the number of words in which the various contents appeared, divided by the total number of words given, thus obtaining the percent. of reactions which the *O* reported in terms of kinaesthesis.[32] This was done to ascertain whether kinaesthesis was distributed through the series or concentrated in one place.

The results show that internal speech occurred with 71 per cent. of all the stimulus words: A reported 75%, G 61%, M 57%, S 96%. Without taking other items into consideration, we

[32] Form partially adapted from Ogden, *op. cit.*, 84.

seem here to be at variance with Ogden, who states that in 67% of his experiments "imageless contents were reported."[33]

(iii) The time reactions of a certain group of words were also studied. From the series in which, according to Table I, *O* reported least kinaesthesis, and from the series where she reported most, some words were taken. These were not intentionally selected, but the first or last few words, in order, were utilized. The results of S were omitted because in her case kinaesthesis was so predominant that the contrast between kinaesthetic and non-kinaesthetic introspections could not be made.

We observed that, with a very few exceptions, in the cases where meaning had been attained without any accompanying kinaesthetic processes, the interval between the stimulus and the response was *less* than 3 sec. In those cases, on the contrary, where introspective reports revealed kinaesthesis, the reaction time was seldom less than 3 sec., and sometimes as much as 29 sec. It seems fair to conclude, therefore, that when the reaction comes quickly kinaesthesis is usually not present, but in longer, *i.e.*, more difficult reactions it is an almost invariable concomitant.

(iv) While internal speech was undoubtedly the most frequent type of kinaesthetic process reported, it was by no means the only form of kinaesthesis present. The following table demonstrates this fact.

Percent. of Kinaesthesis Excluding Internal Speech

O=M

V	IV	I	II	III
3.8	2.7	0	5.8	22

Here again it may be noted that the kinaesthetic processes were most predominant in the most *difficult* series, III.

(v) Affective processes were sometimes reported, especially by M, but were of minor importance, since they usually accompanied the reaction, and were merely expressions of satisfaction or disgust at the response.

(vi) Sensations other than these kinaesthetic processes were seldom noted in the case of A, M and S. G, however, reported a suspension of respiration in the attempt to secure meaning. This was most marked in Series V (30%), where few kinaesthetic processes were reported. For example:

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[33] *Ibid.*, 85.

G IV 15 *Sufokis*[34]
6 *Suffocates*
Repeated *sufokis* IS, and had Vv image of it. Then there was a period of questioning, during which I took two short inspirations. Then came both IS and Vv (in colors)[35] *suffocate, suffocates.*

G V 4 *Onklo*
1.2 *Uncle*
Vv image of uncle accompanied by an expiration.

G VI 8 *Arko*
5.4 *Ark* or *Arch*
Hesitation. Vv arko, then doubt as to whether *E* had said *ako* or *arko*. Vv *ark*. V image of blackness. Pushed breath out in a sort of expiratory pant, and said *ark*.

(vii) Several times *O* reported "conscious attitudes" or *Bewusstseinslagen*. This is the only approximation to Ogden's relational control which in itself "constituted the meaning of the word." But these "conscious attitudes" were usually analyzed out by the *O*s themselves, as for example:

S I 8 *Lerteco*
.8 *Lateral*
Conscious attitude: "Think of something." Feeling of anxiety. Strain in eyes and throat. Awareness kinaesthetically of position of body. IS *lateral*, simultaneously muscular relaxation, and a feeling of pleasantness.

G IV 8 *Kaskado*
1.4 *Cascade*
Vv image (in colors) *cascade*. VC image of *cascade* accompanied by a feeling of assertion. The feeling of assertion is symbolized by a catch of the breath and a contraction of the stomach.

C. General Analysis of Introspective Results. Most of the results have been elaborated in the preceding sections. A few of the introspective reports themselves, however, follow:

A III 7 *Kruta*
1.4 *Crouton*
A image of *crouton*.

M V 6 *Boato*
.97 *Boat*
Vv *boato*. VC image of rowboat.

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[34]In the introspective reports the following form is employed:
1st line—Observer; No. of Series; No. of Stimulus word; Stimulus word.
2nd line—Reaction time in sec.; English word = reaction word.
3rd line—Introspection.
A = Auditory; C = Concrete; IS = Internal Speech (auditory, verbal kinaesthetic imagery); K = Kinaesthetic; V = Visual; ss = sensations; v = verbal.

[35]G has synaesthesia, and sees all words in varied colors. This perhaps accounts for the predominance of visual-verbal images in her reports.

A V 2 *Dio*
God
V image *Deo*, and A image of *God*.

M V 19 *Kampo*
1.31 *Tent*
Vv *campo*, and Vv image of *tent*.

The above introspections are selected because they are a fair sample of cases in which no kinaesthesis at all was reported; *i.e.*, short reaction time, little imagery, suggestion of incompleteness. It will be noticed that three of the four samples were chosen perforce from Series V.

Representative reports from all four *O*s are:

S I 7 *Ferdeko*
1.2 *Dago*
After *E* said *fer* (1st syllable of word), I felt anxiety to know what was coming. Doubt, hesitation, expressed in muscular tenseness. "Would second syllable bring meaning?" Repeated *ferdeko* IS. Then IS *dago*. As word came, had V image of an Italian.

S IV 3 *Cevalo*
1.8 *Chase*
An image of *E*'s word. Then IS *chase*. Until meaning came, there was tension in muscles and K ss. of strain in arms. With meaning came relaxation and feeling of pleasantness.

S I 1 *Skatalo*
3.4 *Scot*
K. ss; strain around eyes. VC image of hand on key. Anxiety symbolized by strain in eyes. Then IS *scot*. After the K image of half-formed word in throat, said *scot*.

S V 5 *Floro*
.88 *Floral*
IS *floro* and *flordalia*. VC image of room in own home, and attempt to make *flordalia* sensible. The elusive familiarity of this word was accompanied by a feeling of unpleasantness and K ss of strain in eyes. Then IS *floral*. After reaction came VC image of a *floral* shop.

The predominance of kinaesthetic processes in the above introspections is obvious. S almost invariably reported strain and tenseness in the period between presentation of the word by *E* and her reaction. Meaning, when it came, was expressed usually in internal speech and was accompanied by a muscular relaxation and some affective feeling.

A V 20 *Vazo*
1.8 *Vase*
K image of position. IS *vazo*. IS *vase*, with vague realization that some people pronounce the *a* broad.

A III 3 *Skurgis*
4.87 *Scourge*
Repeated *skurgis* IS. Then IS *scourge*. This was accompanied by K ss in neck, and K image of position of body, and thrusting forth of head.

A IV 2 *Babili*
1.8 *Baboon*
IS *baboon.*

A II 7 *Atvalora*
2.2 *Ad Valorem*
IS *ad valorem.* Feeling of satisfaction.

A usually reported IS. Her introspections were usually very short, as in examples 3 and 4 above.

M III 13 *Pakas*
3.24 *Package*
Vv *pakas*, repeated IS 1st syllable of word and added *age*. Pressure in arm. With reaction came VC image of brown paper *package.*

M V 1 *Akto*
1.46 *Act*
Vv akto, latin word. Then IS *act.*

M V 8 *Nego*
1.79 *Snow*
Vv image *nego.* Vv image *neige.* Then IS *snow.* With reaction came VC image of white *snow.*

M IV 8 *Kaskado*
2.77 *Cascara*
Vv image *kascado.* Then Av image of *cascara*, and VC image of a pill. After reaction, came VC image of a cascade.

M had a great deal of visual concrete imagery which often accompanied the meaning of the word, especially if kinaesthesis was entirely absent, as in example 4 above.

G III 12 *Sultro*
2.17 *Shoulder*
V perception of apparatus. Inspiration. Tension in abdominal muscles. Felt as if I were sticking my stomach out. Then Vv (in colors) *shoulder*, with fragmentary IS.

G V I *Akto*
Eight
IS and Vv imagery of *ako, eight, octave, akto.* IS esperanto. Probably was word for *eight* in Esperanto. Reaction, accompanied by decided expiration.

G I 1 *Skatalo*
Darkness
Vv image of *skatalo* (blue and green colors). IS *scot.* No sense. "Shall I say it?" Then came Greek word for *darkness, skotos.* Settled down feeling, with muscular relaxation.

G II 1 *Kokcinelo*
14.4 *Cock*
Regarded word as would nonsense syllable. Vv (in colors) *cock*, and *punchinello.* Dubious feeling as to which was correct meaning. IS and fragmentary Vv "probably little *cock* is real meaning." Reacted. Before stimulus word came, had a kind of tickly or breathless sort of feeling located on left side.

G had very vivid visual-verbal imagery, visualizing in color practically every word in every series. The visual-verbal images were often accompanied by internal speech. G also often reported, as above, abdominal, respiratory sensations, etc. When meaning came, *G* usually reported expiratory pant, as has already been noted.

IV. Supplementary Experiment

A. Introduction. This experiment is supplementary to the original experiment already described. The fact that Series III was often more difficult than were I and II was probably due in part to the circumstance that, in the very long words of the latter series, *O* usually seized the first syllable, and gave a meaning to it, instead of to the word as a whole. In an effort, therefore, to make *O* grasp and give meaning to the word in its entirety, the following experiment was tried with the same *O*s under the same conditions enumerated above.

B. Material. *E* prepared three series[36] containing two lists of 10 words each with their English equivalents. The first list in each series consisted of very easy trisyllabic Esperanto words, the English equivalents in one case being disyllabic and in the other two series monosyllabic. The second list in each series consisted of difficult trisyllabic words and their English monosyllabic equivalents.

C. Procedure. The following instructions were read to the *O*s:

> After saying "Ready", "Now", I shall read you a list of ten pairs of words, the second of the pair in each case being the meaning of the first, as: *domus-house*; *chat-cat*. Listen attentively to the series with the definite purpose of grasping and remembering the meaning of each word. Five minutes after the first reading, I shall repeat the series, and allow you to supply the given meaning of each word.

Then to the beats of a metronome (one syllable every sec., with 3 sec. between the words) the list was read to the *O*. After 5 min. rest, during which *O* was instructed not to think of the words just learned, the Esperanto words were again presented in a different order, to prevent serial associations, and *O* was allowed to supply the English equivalent, if she could, and in any case to give her complete introspection. After this was accomplished, the second list was presented in the same manner.

D. Results. In such a short experiment, a statistical analysis of content would of course be insignificant. The following results, however, as far as they go, seem to bear out what has already been noted in the previous experiment.

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[36] See *Appendix*.

(i) Internal speech was very prominent, both in fixing the word when heard the first time, and in bearing meaning the second time. The following introspections bring this out:

S I A 8 *Salono* [*parlor*][37]
1.41 *parlor*
First time I repeated IS *salono*, then IS *parlor*. Tried to connect them. Effort to find synonym. Tension of muscles.
2nd time. IS salono, salon, *parlor*. Reaction.
S I B I *Pokalo* [*cup*]
1.4 *cup*
1st time repeated IS *pokalo*. Strain in arms and elbows and forehead. Tension in muscles of back. Said to myself (IS) *kal-cup*."
2nd time IS *kal-cup* with emphasis on the first letters.
M II B 9 *Vegeti* [*grow*]
3.78 *grow*
Vv image *vegeti*. Repeated twice IS. Feeling of familiarity. Then Vv and IS *vege*. Av image *vegetable*. Then IS *vegetable*, and immediately said *grow*.
S III A 10 *Nebula* [*mist*]
1.21 *mist*
1st time, when *E* presented *mist* as meaning, I said IS "*mist*, not cloud."
2nd time IS *nebulous*. Then VC image of *mist* or grey fog. This came perhaps simultaneously with reaction.
A III A I *Esperi* [*hope*]
1.4 *hope*
1st time associated IS *esperi* with Esperanto.
2nd time repeated IS *esperi*. Then, still in IS, came *hope*. Reacted.
M III A 9 *Promeni* [*walk*]
1.2 *walk*
Vv image *promeni*. VC image of lady dressed in old fashioned clothes, on sunny *walk*. Then IS *walk*.
M I A 10 *Fabelo* [*story*]
1.4 *story*
Vv image *fabelo*. Then IS and Vv image *fable*. Immediately said *story* without imaging it beforehand.
M III B 8 *Klinigi* [*bow*]
polite
1st time before second word was presented, said IS *clinic*. VC image of clinic. After second word, thought "what a funny connection." Then visualized people in clinic bowing to each other and being very polite.
2nd time IS *clinic*. Then came memory image of people in clinic bowing. IS *polite*, and reacted.

(ii) Here, however, as in the other experiment, meaning, it seems, can be carried non-kinaesthetically, especially when easily secured. This is well brought out in the case of G, who because of her long practice in associating words had little difficulty in remembering the meaning of 20 words. Some of her introspections follow:

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[37]The form of these introspective reports is similar to that of the main experiment, *except* that each Esperanto stimulus word is followed in brackets by its English equivalent.

I B 9 *Amelo* [*starch*]
.95 *starch*
Vv image of *amelo*. Reacted, saying *starch*, and afterwards or simultaneously had Vv image of *starch*.
II A 6 *Kompoto* [*jam*]
1.2 *jam*
Reacted immediately and simultaneously visualized (VC) dish of compotes.
III A *Batali* [*fight*]
1.2 *fight*
Vv image of *batali*. Vv battle. Then said and saw (VC) *fight*, simultaneously.
III B 9 *Elporti* [*stand*]
1.4 *stand*
Vv image of *elporti*. Then said *stand*, and saw simultaneously a figure standing. (VC imagery.)

It will be noticed that the above reaction times are very short, as has already been concluded is usually the case when kinaesthesis is absent.

(iii) As the words were more familiar in the second experiment than in the first, it is not surprising that kinaesthesis did not play so important a part, the answering word being often on the tip of *O*'s tongue, so to speak. Though bringing no new facts to light, this experiment, however, seems to strengthen the conclusion already reached. Even under conditions just enumerated, imagery still seemed to play an important part in securing meaning. Out of 180 cases in this supplementary experiment, there was only one instance where *O* did not specifically report imagery.

V. Conclusions and Summary

The dissimilarity, then, between our results and those of Ogden is, of course, obvious. Ogden reports that "in 88 experiments (67.6%) imageless contents were reported. Deducting 6 cases in which concrete visual imagery also appeared, seven in which the meaning of the word was associated with an object present to the senses, and two in which there was definite reference to the direction of an object and therefore a possible kinaesthesis, we have remaining 73 experiments (56.1%) in which no imagery was detected by the observer."[38] In our experiments the number of such cases was found to be practically negligible. In fact, in the supplementary experiment, there was only 1 case out of 180 in which the *O* did not specifically report imagery.

This finding is similar to that of Rogers, who reports that the results of her experiments on perception "agree in general with the view maintained by several writers—James, Bagley, Wallaschek, Lehmann, and Titchener—that the background, sensational or imaginal, is of fundamental importance to the

[38] *Op. cit.*, 85.

meaning of perception. The kinaesthetic and organic sensations and the imaginal processes under our conditions have consistently formed a setting for the central visual complex and have supplied more of the derived significance than the 'directly' initiated processes themselves."[39]

The conclusions of Jacobson, while doubtless more far-reaching than ours, at least agree with our main contention. He found that "the meanings of the stimulus-words were thus carried by visual, auditory, and kinaesthetic processes; or to speak more precisely, the meanings which these processes bore were the meanings of the stimulus -words, in so far as the latter were consciously realized."[40].

As to the nature of the images which play so great a part in the search for meaning, we notice that kinaesthetic images are most prominent. Kinaesthesis, in fact, represented 57% of all images reported. While many forms of kinaesthesis were represented, internal speech occurred most frequently, with 71% of all stimulus words. We did not find, however, that kinaesthesis was necessary to meaning. On the contrary, meaning was sometimes carried by means of both auditory and visual imagery. This is quite in accord with the findings of Ruckmick who stated that "there may be a perception of rhythm without accompanying kinaesthesis, in terms of (*a*) visual imagery, or (*b*) auditory imagery, or sensation."[41]

However, wherever reaction times were long, and the search for meaning was labored and difficult, kinaesthesis was an almost invariable concomitant. It is significant to note here that, in the conclusion to her paper on the analysis of perception, Rogers states that while "we should not go so far as to contend that this 'context' is the meaning, since the process itself is always to be considered as distinct from its function,. . . nevertheless, as meaning becomes more elaborate, the accessory processes acquire increasing importance."[42]

Summarizing briefly, the following tentative conclusions may be stated.

(1) Images play an extremely important part in the search for meaning.

(2) There are individual variations as to the amount and kind of kinaesthesis present, but in some form, and especially as internal speech, it is a noticeable constituent.

(3) Meaning, however, may be carried without any kinaesthesis. In this experiment it was reported as carried in both auditory and visual images.

(4) Kinaesthesis almost invariably accompanies the longer reactions, and a more labored search for difficult and complex meanings.

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[39]*Op. cit.*, 575. [40]*Op. cit.*, 564. [41]*Op. cit.*, 359. [42]*Op. cit.*, 575

APPENDIX

Sample Words from Series Used in Part I

Series	I	II	III	IV	V
(1)	Skatalo	Kokcinelo	Nagos	Agrabla	Akto
(2)	Monaho	Bestkusejo	Fremda	Babili	Dio
(3)	Franajo	Glicirizo	Skurgis	Cevalo	Ruso
(4)	Faceno	Enmiksigi	Glavo	Depeso	Onklo
(5)	Saliko	Proscripcii	Varbi	Festeno	Floro
(6)	Hirundo	Krutegajo	Saumo	Identa	Boato
(7)	Ferdeko	Atvalora	Kruta	Cagrenas	Vino
(8)	Lerteco	Punktokomo	Banas	Kaskado	Arko
(9)	Agado	Tritikajo	Peza	Oferis	Silko
(10)	Frakseno	Malkunigi	Senco	Biciklo	Pluvo

Part II

Series I

	List A			List B		
(1)	Tapiso	—	Carpet	Pokalo	—	Cup
(2)	Angulo	—	Corner	Balai	—	Sweep
(3)	Bileto	—	Ticket	Aleo	—	Walk
(4)	Pasero	—	Sparrow	Makulo	—	Spot
(5)	Citrono	—	Lemon	Sitelo	—	Pail
(6)	Mateno	—	Morning	Oscedi	—	Yawn
(7)	Insulo	—	Island	Forniko	—	Ant
(8)	Salono	—	Parlor	Degelo	—	Thaw
(9)	Argento	—	Silver	Amelo	—	Starch
(10)	Fabelo	—	Fable	Baleno	—	Whale

Series II

	List A			List B		
(1)	Esperi	—	Hope	Elspezi	—	Spend
(2)	Batali	—	Fight	Kuraci	—	Treat
(3)	Aceti	—	Buy	Lacigi	—	Tire
(4)	Elekti	—	Choose	Remuri		Pad
(5)	Konspiri		Plot	Maltrafi	—	Miss
(6)	Ripozi	—	Rest	Aldoni	—	Add
(7)	Dividi	—	Share	Dorloti	—	Pet
(8)	Cagreni	—	Grieve	Klinigi	—	Bow
(9)	Promeni	—	Walk	Elporti	—	Stand
(10)	Deziri	—	Wish	Trastreki		Cross

Series III

	List A			List B		
(1)	Amiko	—	Friend	Maniko	—	Sleeve
(2)	Glacio	—	Ice	Kolombo	—	Dove
(3)	Infano	—	Child	Abio	—	Fir
(4)	Cevalo	—	Horse	Languo		Down
(5)	Almozo	—	Alms	Mentono		Chin
(6)	Kompoto		Jam	Kulero	—	Spoon
(7)	Linio	—	Line	Leporo	—	Hare
(8)	Biero	—	Beer	Paliso	—	Stake
(9)	Objecto	—	Thing	Vegeti	—	Grow
(10)	Nebulo	—	Mist	Timono	—	Pole

TABLE I

OBSERVER	A					G					M					S				
Series	I	II	III	IV	V	I	II	III	IV	V	I	II	III	IV	V	I	II	III	IV	V
Class	%	%	%	%	%	%	%	%	%	%	%	%	%	%	%	%	%	%	%	%
Concrete																				
Visual	14.2	20.8	4.1	4.1	5.8	6	5.8		8.6	6.1	11.1	23.5	24.4	18.9	27.4	6.6	5.8	7.1	1.9	11.4
Auditory																				
Kinaesthetic																				
Processes																				
General		4.1	8.3		2.9	6	2.9	5.4	8.6	4			4.4		1.9	20.0	5.8	11.9	9.8	13.1
Eyes													4.4			16.6	5.8	4.7	1.9	3.4
Ears													4.4				2.9		1.9	3.4
Throat												2.9				16.6	5.8	7.1		1.6
Lips		8.3		8.3														4.7		
Tongue																	2.9	9.5	3.9	3.4
Mouth	7.1			8.3	5.8		5.8											4.7	7.8	3.4
Neck			4.1										4.4							
Head			4.1									2.9		2.7			2.9		1.9	3.4
Arm				4.1		3	5.8	2.7		2			4.4		1.9	6.6	8.8	7.1	9.8	8.1
Verbal																				
Internal Speech	71.4	54.1	70.8	37.5	50.0	27.2	38.2	43.2	28.2	18.3	53.8	28.4	17.3	21.6	13.7	23.3	41.1	38.0	47.0	39.3
Visual				8.3	17.4	54.4	41.1	48.1	52.1	69.3	19.2	38.2	30.4	43.2	41.1	6.6	8.8	2.3	7.8	4.9
Auditory	7.1	12.5	8.3	29.1	17.4						15.3	5.5	4.4	13.5	13.7	3.3	8.8	2.3	5.8	1.6

TABLE II

OBSERVER	A					G					M					S				
Series	I	II	III	IV	V	I	II	III	IV	V	I	II	III	IV	V	I	II	III	IV	V
Class	%	%	%	%	%	%	%	%	%	%	%	%	%	%	%	%	%	%	%	%
Concrete																				
Visual	18.1	33.3	1.9	9.	5.0	14.2	18.1		26.6	8.6	21.4	40.0	58.3	58.3	55	28.5		7.1	25	46.8
Auditory																	9.0			
Kinaesthetic																				
Processes																				
General		8.3	18.0		5.0	14.2	9	14.2	26.6	8.6			18.3		5.0	57.1	18.1	35.8	33.3	26.6
Eyes													18.3			57.1	18.1	7.1		13.3
Ears													18.3				9	7.1		13.3
Throat												13.3				71.4	18.1		25.0	6.6
Lips		18.8		25.0															8.3	13.8
Tongue																	9	7.1	25.0	13.3
Mouth	9.0			8.0	10.0		18.1											28.5	16.6	3.3
Neck			9.0										18.3							
Head			9.0									6.6		9.1			9.0	7.1		3.3
Arm				9.0		7.1	18.1	7.1		4.3			18.3		5	28.5	27.2	28.5	16.6	33.3
Verbal																				
Internal Speech	90.0	83.3	100	55.5	65.0	57.1	100	92.8	66.6	21.7	85.7	60.0	58.3	66.6	30.0	85.7	90.0	100	100	100
Visual			1	18	25	71.4	90.0	92.8	93.3	100	42.8	36.6	91.6	100.0	100	28.5	18.1	28.5	18.0	6.6
Auditory	9.0	16.6	18	41.8	25						28.5	13.3	83.3	41.6	35.0	14.2	27.2	14.2	8.3	6.6

COLOR TESTING AND THE PSYCHOLOGY OF COLOR

By GEORGE HENRY TAYLOR, Railway Medical Officer, Sydney, N. S. W.

Color testing is a factor of importance in the elimination of the unfit in a Railway Service. As my experience of color testing is practically confined to my service as a Railway Medical Officer, a period of nearly sixteen years, my remarks will be founded upon that experience, and as far as possible upon personal observation.

The evolution in the method of color testing in this State is instructive. In 1886 a test for color vision consisting of colored wools was introduced. I have, however, been unable to obtain particulars of the test. When the Railway Commissioners took office in 1888, the then Chief Commissioner (the late Mr. E. M. G. Eddy), who had been an officer of the London and North Western Railway, introduced as a test for color and form vision a card on which were printed four colors, red, green, yellow and blue. Each colored portion measured about $2\frac{1}{4}$ by $1\frac{3}{4}$ in. In the centre of the card were a number of black dots to be used for testing form vision. The examinee was placed at a distance of 15 ft. from the card and was asked to name two colors, the remaining colors being covered by a piece of cardboard. In 1892 the British Board of Trade forwarded for the information of the directors of the various British Railways a copy of the report of the Committee appointed by the Council of the Royal Society (Lord Rayleigh's Committee) and asked for their (the directors') criticism. The report presented to Parliament by the Board of Trade in 1894 shows that the Secretary of the Lancashire and Yorkshire Railway forwarded to the Board of Trade a "copy of a card which is used in the testing of this Company's servants in the matter of color vision." The card, which was reproduced in the report, was identical with that introduced in New South Wales in 1888. In the description and instructions printed in the Board of Trade report appears the following: "Modified after the regulations issued from the Horse Guards by Professor Longmore in 1868." Incidentally, the Secretary of the Lancashire and Yorkshire Railway, in his letter to the Board of Trade, stated: "We find that there are very few cases indeed of color blindness, in fact during the last ten years only one or two men have been found deficient in this respect."

In 1902 my predecessor (the late Dr. G. P. M. Woodward) recommended that the test for color vision should be by means of colored glasses as follows: red, green and purple, as used in

signals, and also a yellow glass. These were placed in a small frame having apertures about 2½in. in diam., made in such a manner as to admit of the glasses being changed by hand, in order to prevent the possibility of their order being learnt. Before approving this recommendation the Commissioners sought the advice of a Sydney ophthalmic surgeon, who endorsed Dr. Woodward's recommendations, and advised that the cards previously mentioned should be used as well as the colored glasses, but that only one color on the card should be shown at a time. These two methods remained in operation until 1904. In that year the question of uniform standards for vision, color sense and hearing was considered by the Interstate Conference of Railway Commissioners, and Holmgren's Wool Test and Dr. Williams' Lantern Test were adopted as the tests for color sense. These tests were brought forward by the then Chairman of the Victorian Railway Commissioners (Mr. Thomas Tait), and were similar to those in use on the Canadian Pacific Railway, of which Mr. Tait had been an officer prior to his appointment in Victoria. The introduction of these tests caused a considerable amount of resentment amongst the employees in the Railway Service, as they resulted in the discovery of several men with a defective color sense. The confusion colors of Holmgren were looked on with suspicion, and eventually the Commissioners decided to replace the Holmgren test for men already in the Service by a test of "ability to select correctly from six shades each of red, green, purple and yellow wools." An examination of a large number of color-blind and color-normal persons by this wool method failed to disclose to the examiner any distinction between the color-efficient and color-inefficient, and it has long since been discontinued in this Service. The Williams Lantern Test was retained.

Some years of experience with the Williams Lantern showed that, owing to the colors being exhibited in the one sequence, it was possible for a person with a defective color sense who had been trained to an exact knowledge of his defect to pass an experienced examiner as having normal color sense, when the lantern was used alone. As a result of consultations between the Signal Engineer of the Department and myself, certain alterations were made in the Williams Lantern, which permitted the examiner to vary the colors exposed, both as regards sequence and combination, as desired.

The Holmgren Wool Test was retained as the test for applicants for employment until 1915. During that time about 58,000 persons were examined, and I was convinced that in a sensible percentage of cases its findings were not correct. Its application was slow and tedious, and it had an exhausting effect on the officers who conducted the examinations.

In 1915, on my recommendation, Stilling's Plates were adopted and used in conjunction with the Modified Williams Lantern as a test for color sense for persons entering the Service.

In 1922 the Interstate Conference of Commissioners adopted Stilling's Plates together with the Edridge Green or Modified Williams Lantern as the test for color sense on all Australian Railways.

I am satisfied that a trained and observant color-blind person may remain undetected in one examination by the Modified Williams Lantern and the Edridge Green Lantern; but I am also satisfied that a person who can pass the Modified Williams Lantern, the colors in which are restricted to red, green and white, without error, and who also satisfies a trained examiner that he is efficient in 1, 2, 3, 4, 5, and 8 of Stilling's Plates, although he may still have an undetected defect, is safe as a railway man under any conditions in which color sense is a guide.

What follows is an example of the unconscious variation through experience in the mind of an examiner in regard to efficiency in color testing by the present method. Out of many thousands of men examined for entry into the Railway Service, the percentage of color failures since 1916, when the method was the Modified Williams Lantern and Stilling's Plates, is:

Year	Failed Stilling & Lantern	Passed Stilling, Failed Lantern	Failed Stilling Only	Total
1916	6.56	2.31	.04	8.93
1917	5.26	1.15	.13	6.55
1918	5.46	1.12	.08	6.67
1919	4.95	.93	.08	5.98
1920	5.20	.16	.02	5.39
1921	4.89	.36	.04	5.30
1922	4.37	.32	.00	4.70

4.70 may be regarded as the percentage of failures of men entering the Service, through color defect.

My experience of color defect includes a personal interview with at least 4,000 red-green-blinds. To explain the inception of my present judgment it is necessary to go a little into detail. During the first seven years of my service in the New South Wales railways as Medical Officer, Holmgren's Wools were a part of the official color test. They were then replaced by Stilling's Plates. Candidates were first taken into the color testing room in batches of four, and there examined one at a time by the wools. After this, they were examined for visual acuity by Snellen's types; and finally, in a naked condition, for physical

fitness. Color blindness debarred a man from work only on the running lines. After a short experience, I detected a difference in the expression of face in a color-blind person from that of a color-efficient person, during my examination by the wools. The color-blind person suggested to me a listening expression, and the color-efficient a watching expression. The general impression that a color-blind face conveyed to me, therefore, as compared with the color-efficient face, was a want in its expression. I may here remark that quite a number of men, including intelligent and educated persons, who are color-blind, are quite unconscious of their defect until it is demonstrated to them. I also noticed that, when selecting the wools in the test, a color-normal person showed much more frequently a slight tremor of his hands, and when spoken to his voice was frequently not under the same degree of control as was that of a color-blind. Of course, a large proportion of the men examined were, from an academic standpoint, uneducated, the percentage of salaried and educated men being comparatively small. In ordinary conversation with a color-blind person his voice is wanting in emotional quality. When he smiles there is an absence of warmth in the smile, and it seems to disappear more quickly than does the smile of a color-normal person. It does not linger for a few seconds in the face. I had difficulty at first, as a type used occasionally to crop up in which the voice and emotional condition closely approximated the color-blind in persons who were color-normal. I ultimately found that this condition was associated with tone-deafness, and at first, even after recognizing this defect, I had great difficulty in separating the one from the other in my mind. However, I now know that the color-blind man has inflection in his voice without warmth; that it does not show an equal response in emotion to the bright side of life, and that therefore its general tone is monotonous as compared with the general tone of the color-efficient voice. The tone-deaf voice has practically no degree of inflection of tone, although it has a quality of modulation. The physical tremor is frequently present in the tone-deaf, quite as frequently as in the color-efficient, and there is more fire in the voice of the tone-deaf when he is roused than in that of the color-blind. By some observers, however, the one will be confused with the other, even when sufficiently educated by experience to recognize the color-blind type. I strongly suspect that Mendel's Law applies to the tone-deaf as it has been proved to apply to the color-blind, and that a similar percentage may be found in each case. There is an obvious psychological analogy between the two conditions. I have never found a man who is color-blind to be also tone-deaf, nor have I met a man who is tone-deaf and found him to be also color-blind. I have never yet examined a woman who was red-

green-blind, and my experience of women is very small as compared with my experience of men; but I have met one woman and one child who were tone-deaf, and it is a little singular that in each case there was a color-blind inheritance on the male side.

Quite a large number of men who are employed away from the lines on this railway are color-blind. I meet these men in my rooms when they are recovering from sickness or when they have been injured; and, as the medical history of each man is with me when I examine him each time, I see and examine him under conditions similar to the color-efficient man. I also know quite a large number of men outside the Service who are color-blind, a number of medical men, schoolmasters, artists, etc.

Some time ago I published in the Lancet and other papers the statement that stammering is much more common amongst color-efficient persons than it is in the color-blind, and this is still my opinion. Since publishing it, I have detected three cases of color-blind men afflicted with a marked stammer; but that nervous quaver in the voice which is frequently in evidence in persons who are a little excited and nervous is almost peculiar to the color-efficient.

I have met men who professed a love of color but who were color-blind, and a case I now quote will show a somewhat similar condition in regard to tone. While I was travelling in a railway train with an intelligent man I have known for some months, our conversation drifted into a discussion of music. My companion stated that he loved music, particularly high-class music, and instanced Mendelssohn's Spring Song as one of his favourite melodies. "However," he said, "my wife will not let me sing as she says I have no sense of tune, and I recognize myself that I have a very bad memory for tunes." I asked him "Are you certain that you have no sense of tune?" He said "My wife, who is a musician, says I have not." I said to him "I will whistle a tune and see whether you can recognize it," and I whistled Mendelssohn's Spring Song, which is also a favourite of mine. "Well," he said, "I think I have heard it before, but I do not know what the name of it is." I then tried him with the Dead March in Saul. He said he thought it must be a march, but he did not recognise having heard it before.

A color-blind person, in my experience, has frequently an intelligent and alert mind. He is stable in his official duty and is not of the type from which emotional firebrands are made. A color-blind to whom color is not interpreted in terms of sight may have a response to what is analogous to color in sight through sound.

THE CAUSE OF THE DISAGREEMENT BETWEEN FLICKER AND EQUALITY-OF-BRIGHTNESS PHOTOMETRY

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The possibility of the use of the eye in the measurement and comparison of light intensities has been a subject of great interest for more than a hundred years. The lack of proportionality in its response to intensity and the variation in this selectiveness of response with wave-length have thus far limited its use to a balancing or equalizing of light intensities. Without an undue amount and detail of calibration it could not be used to measure light intensities directly.

The Eye as a Balancing Instrument. Lights may be balanced either with regard to their energy content or to their power to arouse sensation. While only a limited use can be made of the eye for the former purpose, an important field is found for the latter,—the rating of lights by the eye for its own use, or photometry. However, the employment of the eye for rating lights for its own use is by no means free from difficulty. Here again a selectiveness of response to the different wave-lengths is, depending upon the method used, a serious source of trouble. Important points to be considered in this regard are variations in the intensity of the response with time of exposure and their differences for the different wave-lengths of light. The sensation rises to its maximum through an interval of time and then dies away because of a progressive loss of sensitivity, or adaptation of the eye. Moreover, the rate of rise and fall varies both with the wave-length and with the intensity of light. That is, there are a lag and a decay in the response of the balancing instrument, both of which are variable functions of the composition and intensity of the lights to be balanced. In short, the eye as a balancing instrument may be likened roughly to weighing scales which never quite attain to stability or constancy of balance when the objects or commodities to be balanced are not of the same kind. The result obtained depends upon how long the objects to be balanced remain on the scales, the stability increasing, however, with the increase of the time beyond a certain value, the time required for the instrument to give its maximum response. Moreover, to make the situation still more complicated, the effect on the value of the balance varies with the amount of the commodity present.

From the above considerations it is quite obvious that the length of exposure of the eye to the lights to be balanced is an important factor in the result that will be obtained when these lights differ in composition. The effect of time or length of exposure of the eye as a factor should not be ignored, therefore, in the comparative study and evaluation of the fitness of photometric methods for the purpose for which they are to be used, namely, the rating of lights for the work which the eye is called upon to do. Length of exposure is, we believe, an important, if not the chief point of difference between the two methods of arriving at a balance which are now the most widely used: the methods of flicker and of equality of brightness. In case of the former, a slow succession of exposures is given of a value much less than the time required for the eye to give its maximum response; and in the latter, a single exposure many times greater than the time required for the eye to give its maximum response. When the difference in the way in which the eye is used in the two methods is considered, it is not strange that a poor agreement in result should obtain for lights of different composition. Indeed it is somewhat surprising that agreement should ever have been expected. The reason for the disagreement will become clearer perhaps on a more detailed consideration of the effect of length of exposure on the eye's response.

For exposures less than the time required for the eye to give its maximum response, such for example as are used in the method of flicker, the following facts may be noted. (*a*) The rate of rise differs greatly for the different wave-lengths of light. Also the time required for the sensation to reach its maximum value is short, of the order of tenths or hundredths of a second; and the rate of deviation from equality of response for equal exposures as the sensations rise towards their maxima is rapid. Thus, when it is sought to establish a photometric balance between lights differing in composition by means of exposures of the small values used in the method of flicker, not only will the ratings of the exposed lights sustain a direct relation to the values which the sensations have reached in the individual exposures, and a balance be established which is not a true one for the longer exposures used in the equality-of-brightness method and for the exposures which the eye receives in the greater part of the work which it is called upon to do; but a considerable difference in result should also be expected for small differences in the length of exposure. That is, not only may the balance established by the method of flicker be considered an inapt one for the purpose for which the results are to be used, but it is also subject to no small amount of change by small changes in the speed of rotation of the flicker disc or other apparatus used to present the two lights successively to

the eye. The first of these points is, of course, of the greater importance; the second, the change in result with change of speed, is of consequence, however, because of the difficulty of standardizing the speed of rotation of the disc. The sensitivity of the method requires that the lowest speed be used that gives no flicker. In case the lights differ in color value, this lower limit of speed varies with the different wave-lengths for the same *O* and for the same wave-length for different *O*s, the net result of which is to affect both the amount of the disagreement with the equality-of-brightness method and the precision of the determination. Obviously, too, a difference in result should be expected from any inequality of length of exposure to the two lights, as happens in case of the method of flicker when the rotating sectors exposing the two lights are given unequal values.[1] (*b*) The rapidity of the rate of rise is strongly affected by the intensity of light. The effect of intensity, too, is different for the different wave-lengths. Even the order of ranking of the different wave-lengths as to rate of rise is changed with change of intensity. That is, for a given *O* the rate of rise at a low intensity may be faster for blue and green than for red and yellow; and at a higher intensity this order may be reversed. Intensity of light may thus be expected to be an important factor in any balance between lights of different composition with exposures shorter than the time required for the eye to give its maximum response. (*c*) The state of adaptation of the eye is doubtless a factor in the rate of rise, perhaps also field size, foveal and extrafoveal, although as yet the extent of these influences has not been systematically investigated. (*d*) Individuals differ apparently not only as to the rate of rise, but as to the relative rates of rise for the different wave-lengths.[2] For example, it may be expected that for some *O*s at a given intensity of light the sensations aroused by the short wave-lengths rise more rapidly than those aroused by the long wave-lengths, while for others the reverse of this is true; also that there are individual differences in the effect of intensity of light both on the rate of rise and on the relative rates of rise for the different wave-lengths.

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[1]For a demonstration of this point see A Preliminary Study of the Deficiencies of the Method of Flicker for the Photometry of Lights of Different Color, *Psychol. Rev.*, 22, 1915, 110-162.

[2]This comment is not based upon a direct or systematic investigation of individual and group differences in the rise of sensation, but seems to be a reasonable inference from the collective results of several investigators of this and related functions. It is strongly indicated also by the individual and group results in flicker photometry, interpreted in the light of the results we have obtained with regard to the relation of the disagreement of the two photometric ratings, and the changes and reversals of this disagreement, to the rise-of-sensation curves.

For exposures longer than the time required for the sensation to reach its maximum value, such for example as are used in the equality-of-brightness method, the effect of length of exposure is of comparatively little consequence in the use of the eye as a balancing instrument. (*a*) The rate of decay of sensation is slow. The drop is comparatively rapid just after the maximum is reached, but the curve soon straightens out, becoming less and less steep as the exposure is continued. For exposures of the order of value used in the equality-of-brightness method, the course of sensation is quite stable as compared with the rapidity of change that comes before the maximum is reached. Moreover, the order of magnitude of exposure is that which the eye ordinarily receives in viewing its objects: thus the balance obtained is better suited to the purpose for which it is intended. Also the rate of deviation is slow; therefore small differences in length of exposure have comparatively little effect on the value and precision of the balance obtained. (*b*) Intensity of light has an effect on the rate of decay of sensation, but there is, so far as we can discover, no difference of any very great practical significance in the relative effect in case of different wave-lengths. The reversal, for example, in the relative ranking with reference to rate of change for the short and long wave-lengths, which occurs as an effect of intensity in the rate of rise, never takes place, so far as we know, in the rate of decay. (*c*) There are some individual differences in the relative rates of decay for the different wave-lengths; but again these differences may be expected to be small as compared with differences in the rate of rise.

If the choice were between single exposures of the length ordinarily used in the method of flicker and those of the length used in the equality-of-brightness method, there is, aside from the question of ease of making the determination, little doubt that it would be uniformly in favor of the longer exposures; for there would be just as little reason for rating lights for working purposes with an underexposed eye as there would be for using a false balance as a weighing instrument. There seems to be a belief among some, however, that in the succession of exposures occurring in the use of flicker the impressions summate to such an extent or in such a way as to overcome the unequal effect of the underexposure. To obliterate the unequal effect of the individual short exposures either the sensations must rise to their full value in the succession of exposures or the sensation which lags the most must also summate the faster by an amount which just compensates for its slower rate of rise in the single impression. The former position is out of the question; the latter, as we have shown in a former paper, can not be assumed without violating well-known laws relating to the factors which

influence the persistence of vision. It could not be held, for example, unless it were assumed that the sensation which shows the greater lag and is therefore of lesser intensity should also show uniformly the greater tendency to persist or to carry over to the succeeding impression; which is contrary to fact.

It may still be considered a point for discussion, however, to what degree it should be held that the difference in lag between the sensations aroused by the single exposures used in the method of flicker is obliterated in the succession of exposures. Broadly considered, three positions are possible with regard to this point for the rates of succession employed in the method of flicker. (1) The difference is not obliterated at all. In this case the flicker balance should deviate from the equality-of-brightness balance in direct proportion to the difference in lag for the single exposures. (2) The difference is in part obliterated, but is still present to a degree which renders the method questionable in principle for the rating of lights for the use of the eye when there is a considerable difference in color or composition. And (3) the difference is entirely obliterated, or so nearly so as to be of no practical consequence for the validity of the method. The first is the position indicated by the results to be given in this and a following paper. If there is a summating action due to the succession of exposures, and there probably is, there is no detectable difference in the effect of this action on the sensations aroused by the lights compared. That is, the difference in level to which the two sensations rise in a single exposure is not decreased by a detectable amount by the succession of exposures; for the results given by the method of flicker have been found by us to agree within the sensitivity of the judgment with the results given by the equality-of-brightness method (0.4 to 0.5 per cent for the method of flicker, and 1.3 to 2.7 per cent for the equality-of-brightness method) when the time of exposure by the latter method is made equal in value to the individual exposures in the method of flicker. In other words, the equality-of-brightness method within the limits mentioned gives the same results as the method of flicker when the eye is given the same length of exposure by both methods. This would not be the case (*a*) were the difference in the length of exposure not the cause of their disagreement in result; and (*b*) were the effect of the underexposure on the results obtained by the method of flicker obliterated by any detectable amount in the succession of exposures.

The Behavior of the Eye in Flicker Photometry. The utilization of the absence of flicker as a sensitive means for detecting equality of brightness in two differently colored impressions is based on the following principles. (1) Brightness flicker is due to a succession, at certain rates of speed, of impressions differing

in luminosity or brightness. No flicker occurs, then, when the impressions are of the same brightness. (2) The phenomenon is very sensitive to changes in the luminosity of the successive impressions. (3) Color fusion takes place at a much lower rate of succession than brightness fusion. This leaves the brightness flicker outstanding in a field uniform as to color quality, also of very low saturation owing to the degree of underexposure of the eye.[3]

All of these principles are sound as applied to detecting small differences in the luminosity or brightness of the successive impressions. For this purpose the phenomenon of flicker is excellent; but trouble arises when we try to say that lights differing in composition which arouse sensations of equal brightness for one length of exposure to the eye will arouse sensations of equal brightness for any length of exposure. For change the length of exposure for either the equality-of-brightness or the flicker method, and a different rating is given to the luminosity or sensation-arousing power of the two lights,—inconsiderable, it is true, for the equality-of-brightness method, unless an exposure is used shorter than is required for the sensation to reach its maximum. That is, change the rate of speed of the flicker disc, and a different rating is made of the two lights. Make an equal and corresponding change for the equality-of-brightness method, starting from the same length of exposure as was given by the flicker disc, and a corresponding and approximately equal change in the result is obtained. For example, select a certain rate of speed for the flicker disc, the rate at which the method is most sensitive. Determine the value of the individual exposures at this rate of speed. Set a rotary tachistoscope or other short-exposure apparatus to give an exposure equal to this value, and judge the two lights by the equality-of-brightness method. An agreement of result is obtained within the limits of sensitivity of the method. Increase or decrease the rate of speed of the flicker disc and again determine the value of the individual exposures. Set the rotary tachistoscope to give this value of exposure, and again make the rating by the equality-of-brightness method. Again, an agreement in result is obtained, but the rating by both methods is different from the previous rating. The equality-of-brightness and flicker methods agree in result if the eye is given the same length of exposure to the lights compared in the two methods; they do not agree if the eye is given one length of exposure by the flicker method and another by the equality-of-brightness method. Nor will the equality-of-brightness method agree with itself in any two

[3] As might be expected at the lower rates of succession for which the degree of underexposure is not so great, the colors show a higher saturation.

determinations if in one the eye is underexposed to the lights compared and in the other it is fully exposed as it is in the accustomed use of this method. Moreover, there is no discoverable reason why agreement should be expected in either of the above cases, once the facts about the difference in the variation of the intensity of sensation with the time of exposure of the eye are known for lights of different composition.

The method of flicker would present no difficulty as a means of rating lights with regard to their power to arouse the brightness sensation if the intensity of the brightness sensation were a regular function of the intensity of light. However, the intensity of sensation is an irregular function of three interacting variables: wave-length and intensity of light, and time of action on the eye. It is safe to predict that when the relation of the intensity of sensation to intensity of stimulus is finally and correctly written for vision, intensity of sensation will not be expressed as a regular function of one variable (intensity of stimulus) as it is in the Fechnerian formulation, but as an *irregular function of three variables: wave-length, intensity, and time of exposure.*[4] No one would think of attempting to express the responses of the selenium cell, the photoelectric cell, or the photographic plate, to the different wave-lengths of light as a function of one variable; and yet the eye is admittedly more complex in its responses than any of these reactors.

Factors which Affect the Results in Flicker Photometry. Starting from the above premise as to the influence of length of exposure on the eye as a balancing instrument, it might be instructive to inquire into the number of factors or changes in the working conditions that might be expected to affect the results of the method of flicker. These factors, so far as we have been able to discover, are as follows. (1) Wave-length or composition of light: lights of different wave-length or composition rise to their maximum value in sensation at different rates, *i.e.*, they show different amounts of lag. Lights of different composition which arouse sensations of equal brightness at the optimum value of exposure will arouse sensations of unequal brightness for the short exposures given by the sectors of the flicker disc run at the speeds ordinarily employed. (2) Intensity of light: the lag changes with change of intensity of light and the change is different in amount for the different wave-lengths. The effect of change of intensity of light on the sensations aroused by exposures of the length used in the method of

[4]Even when the wave-length and exposure are held constant, the intensity of sensation is not a regular function of intensity of light, more particularly when short exposures are used. The deviations from regularity are quite large when the eye is exposed for a time shorter than is required for the sensation to reach its maximum.

flicker is to change the relative levels to which the sensations aroused by the lights compared are allowed to rise, and thus the value of the balance which is established by the method of flicker. (3) The rate of speed at which the flicker disc is rotated: a change in the rate of speed changes the value of the individual exposures, and this changes the relative levels to which the sensations compared are allowed to rise during the individual exposures. Also this change might be expected to have, and does have, a different effect for different intensities of the same pair of lights. (4) The relative lengths of exposure given by the lights compared, or the relative values of the open and closed sectors of the disc. In practice the open and closed sectors are given the same value. However, if these values are made unequal, the levels to which the sensations are allowed to rise change correspondingly, and the relative rating of the two lights as to luminosity should be changed. For example, if an *O* is used who underestimates red and yellow and overestimates blue and green at a given intensity of light by the flicker method, a change in the value of the sectors to give a shorter exposure to red or yellow, and a longer exposure to blue or green, should cause a greater underestimation of red or yellow and a greater overestimation of blue or green. (5) Field size may be, and probably is, a factor insofar as changes in the field size may affect the relative rates of rise of the sensations aroused by lights of different composition. The effect of field size on the rate of rise of sensation presents an interesting subject for further investigation and one which, so far as we know, has not been undertaken. (6) State of adaptation of the retina: this may also be a factor, through a possible influence on the rate of rise of the sensations aroused by lights of different compositions.

Results showing the effect of the first three of the above factors will be given in later papers. Accompanying these results, curves will also be given showing the rate of rise of sensation plotted in just noticeably different steps (the time j.n.d.) for the same *O* at each of the intensities and compositions of light employed in the photometric work.

Difference in Length of Exposure the Cause of the Disagreement between the Flicker and Equality-of-Brightness Ratings. The evidence in support of this point may be summarized as follows.

(1) Our first and more fundamental evidence is the comparison of the type and amount of the disagreement between the two methods with the brightness which the sensations have been allowed to attain for an exposure time equal to that of the individual exposures used in the method of flicker. As a basis for this comparison the brightness was measured in terms of just noticeable differences from zero. A close agreement was found.

The rise of sensation curves was determined with the same apparatus, the same *O*, the same composition and intensities of light, and the same state of adaptation as were used in the flicker and equality-of-brightness comparison.

(2) The effect of intensity of light on the type and amount of the disagreement was determined, and the result was checked up on the rise of sensation curves for three of the seven intensities used. Again the disagreement was found to sustain a close relation to the difference in height to which the sensations are allowed to rise for these intensities with the lengths of exposure used in the method of flicker.[5] Furthermore, an intensity was found for each pair of lights at which agreement occurs for the two methods of photometry with the speed of rotation of the disc ordinarily employed. Above and below this intensity came underestimation or overestimation of the colored light, depending upon the color selected for comparison with the white light. No single intensity could be found at which agreement occurred for more than one pair of lights. These most favorable intensities were quite widely separated for the four pairs of lights, and the percentages of disagreement at each of these intensities for the three remaining pairs were quite large.

(3) The effect of speed of rotation of the flicker disc on the type and amount of disagreement was determined for each of the intensities of light employed. Changes in the speed of rotation of the disc change the length of exposure and therefore the height to which the sensations compared are allowed to rise. A number of speeds was used. A very considerable effect on the disagreement was found, varying with the intensity of light employed. At the intensity at which agreement occurred for the two methods at the most sensitive flicker speed, variation in speed produced its least effect, as might be expected from the small angle of deviation of the rise of sensation curves for the lights compared at their point of intersection. In some cases the closest agreement came when the speed was low, in others when it was high. The result would obviously depend upon the relation of the rates of rise of sensation for the two lights above and below the points from which the change of speed was made. In much the greater number of cases, however, the closest agreement came with the slower speeds and the longer exposures. Considering the trend of results very broadly, it may be said

[5] A comparison of the correspondence between the two types of photometric rating and the differences in height to which the sensations have been allowed to rise for the given exposures can be made with exactness only with regard to the type of disagreement and the order of ranking as to amount. Comparisons of actual amounts of disagreement with differences in sensation level must necessarily be rough.

that, as the speed is decreased and the length of exposure increased, agreement is approached irregularly as a limit, the limiting value of exposure being that used in the equality-of-brightness method.

(4) The effect of varying the relative values of the exposure to the lights compared was determined. The effect of this unbalancing of the flicker photometer, if a disturbance in the adjustment of an instrument already out of balance can be called unbalancing, varies both with the intensity of the light and with the speed of rotation of the disc. In general, the effect of a relative decrease of the exposure to a light already underestimated and an increase in the exposure of the light with which it is compared tends to increase the amount of the underestimation of that light by the method of flicker, and conversely. It would seem that a degree of physical unbalance or ratio of open to closed sector might be found, for any given intensity of light and speed of rotation of the disc, that would give agreement between the two methods, or would even change an underestimation to an overestimation. This latter point has not yet been verified by us.

(5) Each pair of lights at three intensities was rated by the equality-of-brightness method with a value of exposure equal to that of the individual exposures used in the method of flicker. These short exposures were made by means of a rotary tachistoscope. Agreement of result was obtained within the limits of sensitivity of the judgment. In every case the flicker result fell within the small range of values that was judged equal by the equality-of-brightness method. This might well be considered the most important step in the argument. It was added as a final confirmation, not only to show that difference in time of exposure is the cause of the disagreement between the two methods, but also to ascertain whether there is any detectable effect of summation on the disagreement, due to succession of impressions given in the method of flicker; that is, whether the balance which would be made for a single exposure is modified by the succession of exposures. If agreement in results is obtained by the two methods under this condition it would seem reasonable to conclude that no considerable differential effect could be attributed to summation. The judgment by the equality-of-brightness method for the short exposures was not as difficult as might be supposed, because the color difference was very much reduced by the brevity of the exposure; nor was the sensitivity of the method reduced to any considerable extent by the shortness of the exposure.

The lights used in these studies differed greatly in composition. The colored lights were selected from the spectrum of

a Nernst filament by a slit 0.5 mm. wide, and all trace of impurities was absorbed out by means of thin gelatines placed over the objective slit. They were red 675$\mu\mu$; yellow 579$\mu\mu$; green 515$\mu\mu$; and blue 466$\mu\mu$. Seven intensities were used, ranging from 12.5 to 50 meter-candles; the field size was 1.9°. The standard lamp was a 32 c.p., 4.85 watts per mean spherical candle, carbon lamp. When lights so different in composition as these are used, there can be no doubt that there is a disagreement in result by the two methods of photometry of a type and amount that cannot be ascribed to uncertainty of the judgment. The consistent throw of the disagreement in one direction or the other, for a given *O* for a given intensity and difference of composition of light, by an amount greatly in excess of the mean variation by either method indicates clearly that there is a physiological basis for the disagreement. For example, if the disagreement were due merely to an error or uncertainty in the judgment, the throw for a given *O* would be as likely to be in one direction as the other. Or if the disagreement were due to a difference in the pattern or concept of what is equality of brightness in the presence of hue difference, as some have suggested, there should not be a complete and consistent reversal in type of disagreement for the same *O* for the same pair of lights and hue difference with no other change of conditions than change of intensity. A change or difference in the subjective pattern could account only for such phenomena as inconsistency of rating by the equality of-brightness method for the same *O* at different times, and for differences in rating by different *O*s. A consistent throw in any direction by the same *O* at widely separated intervals, and an equally consistent reversal in the direction of the throw with change of intensity, could scarcely be accounted for on the ground of unreliability or inconsistency of the judgment due to an instable or fluctuating pattern. The disagreement is unquestionably physiological, not mental,—grounded in the nature of the response of the balancing instrument, not in the evaluation of that response by the judgment. It would seem just as plausible to ascribe the Purkinje shift and other phenomena, well recognized and established as optical, to an error of judgment as this equally consistent disagreement of the flicker and equality-of-brightness methods. The facts are that we have one type of effect of change of intensity on the eye as a balancing instrument when exposed for 0.5 sec. or more, and another when exposed from .015 to .025 sec. The two effects do not run parallel. They would run parallel, or approximately so, if for a given difference of wave-length and of time of exposure the response of the eye were a regular function of intensity. That is, even with wave-length and time of exposure held constant, the

response of the eye is an irregular function of intensity, quite strikingly so when the time of exposure is less than that required for the sensation to rise to its maximum.

Individual and Group Differences in Flicker Photometry. That there is a great deal of difference in the behavior of the flicker photometer with different *O*s is well known. The most that can be hoped is that *O*s will ultimately be found to fall into rather large groups or types. This tendency to group according to type was brought out clearly in the work of Ives and Kingsbury[6] and of Crittenden and Richtmyer.[7] It has also come out strongly in the drill work among our own students during the course of several years. For the *O* used in these experiments, red was overestimated at 50 m.c., underestimated at 25 and 12.5 m.c., and agreement with the equality-of-brightness method came at 29.39 m.c. But there is no guarantee whatever that this result will be true for another *O* picked at random. A group of *O*s, however, may be found which shows roughly the characteristics of this *O*. A fair percentage of such *O*s has been found among the students of our laboratory. As yet, however, we have not had the opportunity to compare their flicker results with those for the rise of sensation. In relation to explanation, obviously a study should be made of individual and group differences in the rise of sensation. The work on rise of sensation, so far as it has been carried, seems to indicate that great individual differences may be expected.

The Rating of Lights in Relation to their Service to the Eye. The question may be raised how much lights must differ in composition before the disagreement between the flicker and equality-of-brightness ratings comes to be a serious matter in practical work. Is the discrepancy great enough to be of considerable consequence for the range of difference of composition found among artificial illuminants? Comparisons of artificial illuminants have been made by Wilde,[8] Bell,[9] Crittenden and Richtmyer[10] and others. It has not been our purpose in this series of studies to go into that question at all. Our object has been merely to make an analytical and explanatory study of the phenomenon of flicker under various conditions pertinent to its application as an indicator in making a photometric balance.

While it may be said that the equality-of-brightness method gives a rating for exposures more nearly in accord with those

[6]H. E. Ives and E. F. Kingsbury, *Trans. I. E. S.*, 10, 1915, 203.

[7]E. C. Crittenden and F. K. Richtmyer, *Trans. I. E. S.*, 11, 1916, 331.

[8]L. W. Wilde, The Photometry of Differently Colored Lights, *The Electrician*, 43, 1909, 540-541.

[9]L. Bell, Chromatic Aberration and Visual Acuity, *Electrical World*, 57, 1911, 1163-1166.

[10]E. C. Crittenden and F. K. Richtmyer, *loc. cit.*

ordinarily used in the act of seeing than does the method of flicker, and should therefore be regarded as the logical standard of photometric methods, still, as was shown in a former paper,[11] even the equality-of-brightness method does not rate lights differing widely in composition according to the power they give the eye for clear seeing. That is, it was demonstrated in that paper that spectrum lights, made photometrically equal by the equality-of-brightness method, give rather widely different acuities, speeds of discrimination, and powers to sustain acuity. Furthermore, a continuation of that study has shown that spectrum lights made equal both as to saturation and brightness also give differences in acuity, though not nearly so great. Obviously, a method based on some aspect of acuity would rate lights differing in composition more nearly in accord with their serviceability to the working eye than any method in which the ratings are made in terms of brightness. Acuity, however, is an insensitive indicator of changes in light intensity at medium and high illuminations. Perhaps the needed sensitivity could be added by using speed of discrimination as an indicator, instead of acuity as determined in the conventional way. While acuity changes slowly with intensity at medium and high illuminations, speed of discrimination changes rapidly with increase of illumination at these intensities and very rapidly indeed at low illuminations. The rating of lights in terms of their ability to give equal speeds of discrimination of a 1′ opening or less in a broken circle, or some other suitable detail, would not be a difficult or infeasible task. Nothing more would be involved in that task than indicating which way the opening was turned; and the judgment, moreover, would present no more difficulty for lights differing in color than for lights of the same color. The method too would have the very great advantage of an objective check on the correctness of the judgment.[12] Also acuity itself

[11]C. E. Ferree and G. Rand, The Effect of Variation of Visual Angle, Intensity, and Composition of Light on Important Ocular Functions, *Trans. Ill. Eng. Soc.*, 17, 1922, 69-102.

[12]If at high intensities the speed of discrimination for the size of test-object used should be less than the time required for the sensation to reach its maximum, the objectionable effect of underexposure could be eliminated by having the surface covering the test-object of the same coefficient of reflection as the test surface. This preexposure, which could be made of any length desired, would add to the time the light acted on the eye. The eye would thus not be underexposed to the light which is being rated. That is, the short exposure would apply only to the discrimination of detail at a given level of brightness, not to the brightness which would be aroused. Again, the smaller the test-object, the longer would be the exposure needed to discriminate the required detail. This principle also could be used, if desired, to guard against underexposure. The decrease in the size of the test-object, by increasing the difficulty of the task set to the eye, would, moreover, add to the sensitivity of the method for picking up differences in intensity.

changes rapidly at low illuminations. That is, a method of rating lights based on acuity at low illuminations would have a great deal of sensitivity. If, as is stated by some writers, the Purkinje phenomenon is absent in the fovea, advantage could be taken of that fact always to make the ratings at low illumination. The detail to be discriminated would always fall well within the fovea. It might be well to revive the acuity method and to see what can be done with it in terms of facts that have been brought out in recent work. Where the task set to the eye is merely the discrimination of brightness, it is clear that the rating of the lights to be used should also be in terms of luminosity or brightness; but where the task is the discrimination of the form or detail of illuminated objects, the rating of the lights in terms of some aspect of acuity would be more appropriate to the use which they are to subserve.

Theories as to the Cause of the Disagreement between the Flicker and Equality-of-Brightness Methods. Three factors have in the main been assigned as the cause of the disagreement between the flicker and equality-of-brightness ratings: the effect of intensity of light, the size of the photometric field, and the time of exposure of the eye to the lights compared. Lauriol,[13] Dow,[14] Millar,[15] Ives,[16] and Luckiesh[17] have investigated the effect of intensity of light; Schenck,[18] Dow,[19] and Ives the effect of size of field. With regard to intensity of light as a factor, Lauriol and Dow claim that the relative shift in the brightness of the different colors at low illuminations is shown by both methods. The shift for Dow, however, is more pronounced in the equality-of-brightness than in the flicker determinations. For Lauriol the shift for the different colors varies in magnitude by the two methods and in some cases in direction. Millar, on the other hand, photometering a mercury against a carbon lamp, claims that the Purkinje phenomenon is not shown at all by the flicker method at low illuminations; while Ives and Luckiesh go

[13]Lauriol, Le photomètre à papillotement et la photométrie heterochrome, *Bull. Soc. Intern. des Électriciens*, 1904, 647-652.

[14]J. S. Dow, Color Phenomena in Photometry, *Philos. Mag.*, 12, Ser. 6, 1906, 131.

[15]P. S. Millar, The Problem of Heterochromatic Photometry, *Trans. I. E. S.*, 4, 1909, 769.

[16]H. E. Ives, Studies in the Photometry of Lights of Different Colors, *Philos. Mag.*, 24, Ser. 6, 1912, 149-188.

[17]M. Luckiesh, Purkinje Effect and Comparison of Flicker and Equality of Brightness Photometers, *Electrical World*, March 22, 1923, 620.

[18]F. Schenck, Ueber die Bestimmung der Helligkeit grauer und farbiger Pigmentpapiere mittels intermittierender Netzhautreizung, *Pflüger's Archiv*, 64, 1896, 607-628.

[19]J. S. Dow, *op. cit.*, 130-134; Physiological Principles Underlying the Flicker Photometer, *Philos. Mag.*, 19, Ser. 6, 1910, 58-77.

to the other extreme, and declare that a reverse Purkinje effect is obtained by the flicker method.

Dow, a follower of the *Duplizitätstheorie* of color vision, believes that there are two kinds of flicker, a coarse flicker (rod flicker) and a fine flicker (cone flicker). The final adjustments for the photometric balance by the method of flicker are made by the fine or cone flicker. The photometric balance, then, by the method of flicker, is always in terms of cone functioning. The equality-of-brightness balance, however, is in terms of both rod and cone functioning, unless a field size is chosen which falls within the fovea. If a field size is chosen which falls within the fovea the balance is established by both methods in terms of cone functioning, and a substantial agreement in result should be expected.[20] These explanatory principles proposed by Dow, although not completely worked out and elaborated by him, may be fairly singled out, we think, as one type of theory as to the cause of the disagreement of the results by the two methods.

Space will not be taken to develop all of the arguments against Dow's conception of rod and cone functioning as the cause of the disagreement. However, since the importance of intensity as a factor has been noted by him and other writers, it might be well to consider whether its effect can be explained in terms of the rod and cone hypothesis. If the sole cause of the disagreement is in terms of a difference in rod and cone functioning, it is not readily seen how intensity could affect the disagreement by the two methods at all when a field size is chosen which falls within the fovea; for with fields of this size the balance is established in terms of cone functioning by both methods; and intensity, if it affects the result by one method, should correspondingly affect the results by the other. The effect of intensity, however, is not only readily explained as an effect of lag on the balance established by the method of flicker, but it is just what would be expected from a study of the curves for the rise of sensation for different intensities of light. Even the disagreement as to the effect of the change from high to low intensities found by Lauriol, Dow, Millar, Ives and Luckiesh is what might be expected in terms of this explanatory principle. That is, these men probably worked with different types of O,—Ives and Luckiesh with a type for which it might be inferred that red and yellow rise to a higher value than blue and green for the length of exposure used in the method of flicker for the lower intensities,[21] and the converse for the higher intensities. There seems to be no explanation at all for such differences in effect in terms of the Dow adaptation of the *Duplizitätstheorie* of color vision.

With regard to size of field as a factor, Schenck found that a decrease in size lowered the mean variation for the flicker method and decreased the luminosity value for all colors. Dow found that as the size of field was decreased red and yellow lightened relatively to blue and green. Ives,[22] admitting the disagreement and accepting size of field and intensity of stimulus as important causes, sought to determine whether a field size and an intensity could be found for which the methods agree. He photometered different portions of the spectrum against carbon lamps at a number of intensities and with different field sizes, and found in general that the luminosity curves obtained by the two methods differed. The difference,

[20]Also it may be pointed out that, in terms of the theory, the proportion of cone functioning would increase with increase of intensity of light for extrafoveal field sizes.

[21]For an example of this type of O, see M. A. Bills, The Lag of Visual Sensation in its Relation to Wave-Length and Intensity of Light, *Psych. Mon.*, No. 127, 93.

[22]H. E. Ives, *loc. cit.*

however, was less for high intensities and small field sizes than for low intensities and larger field sizes. He concludes that high illuminations (25 m.c. and above) are the most favorable to agreement.[23] Our results seem to indicate that perhaps this generalization was based on the use of too limited a range both of the higher intensities and of wave-length. Agreement was found by us for the spectrum red ($675\mu\mu$) and the carbon lamp at 29.39 m.c.; for the spectrum yellow ($579\mu\mu$) at 26.12 m.c.; for the green ($515\mu\mu$) at 11.91 m.c.; and for the blue ($466\mu\mu$) at 14.91 m.c. Twenty-five m.c., the value which he estimated his most favorable intensity approximately to be,[24] was found by us to be comparatively favorable to agreement for red and yellow (the range of spectrum used by Ives was 653 to $517\mu\mu$); but the disagreement for these colors became large again as the intensity was increased up to 50 m.c. Moreover 25 m.c. was not at all favorable for the blue and green. The most favorable intensity is the one which allows the sensations aroused by the lights compared to rise to the same level or nearly so for the short exposures used in the method of flicker. Just what this intensity is can not be predicted in advance for any *O* even for one pair of lights.

A table is appended, Table I, in which is shown in percentages the difference in result obtained by the five *O*s used by Ives at the intensity of light (250 Illumination Units) which of those used he calls most favorable to agreement by the two methods. It will be seen from this table that the disagreement is by no means eliminated. It will be seen also that for each *O* the closest agreement came at one particular point in the spectrum and fell off on either side of this, the disagreement becoming quite large as the distance from this point increased. In this table percentage overestimation is designated by + and underestimation by −.

It seems quite clear that field size and intensity can not play a very important rôle as a foundation cause of the disagreement in the results of the two methods, and that agreement can not come for lights of all compositions through a search for one most favorable field size and intensity. We have found only one way in which agreement can be brought about for lights of all compositions, namely, by giving the eye the same length of exposure by both methods. When an exposure is selected for the equality-of-brightness method equal in value to that given by the individual exposures in the method of flicker, the desired agreement alone is obtained. When, however, agreement is wanted only for a given pair of lights, the case is different. An intensity can be found that will satisfy this condition, namely, that intensity for which the sensations aroused by the two lights have risen to the same level for the lengths of exposure given in the method of flicker. Such an intensity has been found by us for each of the four spectrum lights when matched against the carbon lamp. This intensity, however, is different for each of the spectrum lights.

That time of exposure is the cause of disagreement was, so far as we know, first suggested by Morris-Airey. It would seem *a priori* that time of exposure might affect the results of the method of flicker in one or both of two ways,—through a difference in the rise of sensation for the different

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[23] "Of considerable practical interest is the fact that the flicker method most nearly agrees with the method of equality of brightness at high illuminations" (Ives, *Philos. Mag.*, 24, (6), 1912, 182). "An illumination of 25 c.m. was chosen since by the means described below this was found to correspond closely to the high illumination at which the results of the flicker and equality of brightness methods became the same" (*op. cit.*, 854).

[24] H. E. Ives, The Spectral Luminosity Curve of the Average Eye, *Philos. Mag.*, 24, (6), 1912, 853-863.

TABLE I[25]

Showing in percentage the difference in result between the methods of flicker and equality of brightness for the five *Os* used by Ives at "250 Illumination Units", his most favorable intensity

λ	H.E.I.	M.L.	P.W.C.	C.F.L.	F.E.C.
.653	−12.0%	+29.0	−18.0	−51.0	−50.0
.643	− 3.6	+56.0	− 7.0	−31.0	−23.7
.632	− 4.3	+20.0	−15.5	−45.5	−12.9
.622	− 7.3	+12.5	− 4.2	−42.0	−15.9
.612	−10.0	+ 8.3	− 6.5	− 7.5	+ 0.3
.594	− 1.0	− 0.5	− 0.5	+ 7.5	+ 5.8
.574	+ 0.5	− 2.4	− 2.5	+27.0	− 5.9
.555	− 0.4	− 8.0	−11.9	− 4.8	+ 8.9
.545	− 3.1	− 8.4	−13.8	+ 3.0	+ 8.0
.535	− 1.9	− 4.0	−12.6	−12.0	+14.3
.526	0	− 8.7	−21.4	− 3.0	+35.5
.517	+ 0.6	−10.8	−13.8	−33.0	+30.0

colors during the individual exposure, and through a difference in their persistence between exposures. Time of exposure might affect the results by the equality-of-brightness method through a difference in the decrease or decay of sensation when the eye is exposed through a longer period than is required for the sensation to reach its maximum, although the chances of this effect being of any considerable importance are slight owing to the comparatively slow rate of decrease of sensation after the maximum has been reached. A brief discussion of how time of exposure might be expected to be effective is given by Morris-Airey, but no experimental work was done. In an article appearing in the *Electrician*, 64, Dec. 17, 1909, 401, he says: "In recent years photometers of the flicker type have obtained a certain degree of prominence, but from time to time suspicions have been raised as to whether the principle on which flicker photometers are based does not involve physiological phenomena which disturb the conditions of illumination so that the numbers obtained are not a true representation of the illuminating powers of the sources to be compared. Physiologists are at variance with regard to color theory. ..... With every new change of color theory the physicist would have to change his starting point in the explanation of a phenomenon like the flicker effect and I suggest that a more stable foundation for a physical theory is to be found in the experimental examination of the growth and decay of the retinal stimulus due to differently colored lights without any attempt to connect the form of the curves obtained with physiological theories which are admittedly of uncertain stability." Morris-Airey then cites a phenomenon described by G. N. Stewart (*Proc. Roy. Soc. Edin.*, 1888) which is merely a re-discovery by Stewart of a phenomenon described by Fechner[26] many years before and now called Fechner's colors. Stewart observed that when a mirror reflecting white light was rotated at different rates of speed, the image of the source of light appeared successively in different colors. This selective unbalancing of the retina's action to give the component colors instead of white obviously could be due either to difference in rate of rise of the component color excitations or to a difference in their rate of persistence through the dark interval between the flashes, or to both. This phenomenon can be more conveniently demonstrated by the rotation of either white or black

[25]These percentages were computed by us from Table II, *Philos. Mag.*, 24, (6), 1912, 186.

[26]G. T. Fechner, Ueber eine Scheibe zur Erzeugung subjectiver Farben, *Poggen. Annal.*, 45, 1838, 227-232.

sectors (180° each), a black spiral on a white disc, or a specially constructed disc (Benham's disc) made up of sectors of 180° of white and 180° of black. On the white sector are painted narrow 45° arcs at different distances from the center of the disc and differently spaced with relation to the edges of the white sector. These black arcs are seen in different colors depending upon their spacing in relation to the edges of the white sector, and the colors change as the speed of rotation of the disc is changed.

The rate of growth and decay as an explanatory principle was considered by Ives,[27] who seemed to believe, however, that Dow's rod and cone theory offered greater possibilities of explanation. As an objection to the explanation suggested by Morris-Airey it is interesting to note that Ives says: "It is not established that at the same illumination the rates of rise of sensation are actually different for different colors.....it is clear that these phenomena of rise of sensation must be studied with the same observer and apparatus by which are made the flicker measurements in which they may play a part." In Jan. 1914 the theory advanced by us in this paper was first proposed before the Philadelphia Section of the Illuminating Engineering Society, and experimental data were offered in its support. This paper was reviewed in an editorial in the *Electrical Review and Electrician*, Mar. 7, 1914, 478-479; and was published in full in the *Psychol. Rev.*, 22, 1915, 110-162. Later in 1914 Luckiesh[28] explains the discrepancy between the flicker and equality-of-brightness ratings in terms of the different rates of growth and decay of the color sensations; but he seems inclined to reject this explanation in an appendix to the same paper. Still later in the same year Ives (*Philos. Mag.*, 28, (6), Nov. 1914, 708-728) published an article in which a theory of flicker photometry is developed based on an analogy drawn between the response of the eye under successive stimulation and the action of incandescent lamp filaments under a fluctuating current. In this article an attempt is made to show mathematically that, if the eye behaves under the conditions obtaining in flicker photometry as do lamp filaments (subject to certain modifications) under a fluctuating current, the method of flicker should give with high intensities of light at the photometer screen the same results on the average as the equality-of-brightness method. The analogy of the eye to the incandescent lamp filament is not based on an experimental examination of the eye's manner of response, but is assumed. A belief in the importance of lag as a factor comes out clearly in his later writings. He says (*Philos. Mag.*, 33, (6), 1917, 19-33): "The chief experimentally established phenomena of the flicker photometer find explanation in the assumption that the stimulus is transmitted through a layer of matter having a coefficient of diffusivity which is different for the different colors and varies with the intensity of the stimulus.....The interval between the reception and the perception of the stimulus [lag] is inversely as the diffusivity."

The theory proposed by us differs from that suggested by Morris-Airey and later by Luckiesh in that we attribute the cause to differences in the rate of rise of sensation, not to both rate of rise and decay. Difference in rate of decay as a factor is, we grant, an *a priori* possibility; but we feel reasonably sure that it is not a factor of any considerable consequence; because, if it were, its effect would be manifested through a differential

[27] H. E. Ives, *Philos. Mag.*, 24, (6), 1912, 178-181.

[28] M. Luckiesh, *Phys. Rev.*, 4, 1914, 1-11.

summation effect. That there is no detectable differential summation effect seems pretty well proved by the fact that within the limits of sensitivity of the equality-of-brightness judgment the same results are given by both methods of rating when the same lengths of exposure are used. This does not mean, of course, that neither sensation has risen to a higher level in the succession of exposures than it has attained in the single exposure. It means only that, in rising, the relation of level has not changed by a detectable amount,—in other words, that the sensation which lags the more in the single exposure has not gained on the other sensation in the succession of exposures.

FLICKER PHOTOMETRY AND THE LAG OF VISUAL SENSATION

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The object of this paper is to show the relation of the lag of visual sensation to the disagreement of flicker and equality-of-brightness photometry for lights of different composition.

The experimental work covers two points: (1) a comparison of the rating of four spectrum lights at three intensities against a carbon lamp by the methods of flicker and equality-of-brightness; and (2) a determination of the curves of the rise of sensation for these five lights at each of the three intensities, with the same apparatus, the same O and the same state of adaptation of the eye, as were used for the photometric comparisons. The results obtained enable us to make a direct comparison of the type and roughly of the amount of the disagreement between the two methods of photometry with the actual luminosities or brightnesses to which the two sensations had been allowed to rise for values of exposure equal to the individual exposures used in the method of flicker. As a basis for this comparison the brightnesses were measured in terms of just noticeably different steps from zero. No experimental evidence more direct as to type than this can be offered, we believe, on the relation of lag to the disagreement in question. If it be found, for example, that for a given intensity of light, red and yellow are underestimated by the method of flicker and blue and green overestimated, then it should be shown by the curves that the sensations aroused by the red and yellow lights had risen to a lower and by the blue and green to a higher value than the sensation aroused by the light of the carbon lamp. The value which the sensation would attain can be read off directly from the curve, and comparisons can be made with the type and amount of disagreement between the two methods of photometry. In a third paper, the evidence offered by this comparison will be supplemented and rounded out by results when the eye is given the same length of exposure by both methods of photometry, and by showing the effect of varying the speed of rotation of the flicker disc and the intensity of light on the amount and type of disagreement between the results in question.

The colored light was taken from the spectrum of a Nernst filament operated at 0.6 amp.; a narrow band in the red at 675 mμ, of yellow at 579mμ, of green at 515mμ, and of blue at 466mμ. The breadth of slit used in isolating these bands was 0.5 mm.

In every case the light was examined for impurities at the objective slit by means of a small Hilger direct-vision spectrometer provided with an illuminated scale. When found, impurities were absorbed out by thin gelatine filters placed over the objective slit, so selected as to absorb as little as possible of the useful light. The spectrum lights were photometered against a 32 cp. carbon lamp, 4.85 watts per mean spherical candle. The photometric comparisons were made and the rise of sensation curves determined at 12.5, 25 and 50 m.c. as measured by the equality-of-brightness method.

The plan of apparatus used is indicated in Fig. 1 and a photograph is shown in Fig. 2. The light from the objective slit was collimated by the lens A, Fig. 1, and focussed on the eye by the lens B. The image formed fell well within the pupil. By substituting this lens system for the eyepiece of the spectroscope, the need and inconvenience of the use of an artificial pupil was eliminated and a surface of uniform color was presented to the eye. Lens B was diaphragmed by a circular opening E of such a size and such a distance from the eye as to furnish a photometric field of 1.9 degrees. Between the lenses A and B was interposed the 90° sectored disc D mounted on an electric rotator; and between lens B and the opening E, the disc F of the rotary tachistoscope used in making the exposures for determining the rise of sensation curves. Discs D and F were both of light sheet aluminum. Disc D was coated with magnesium oxide deposited from the burning metal; disc F was painted matt black.

The method of making the photometric comparisons was as follows. For the equality-of-brightness determinations, disc D, reflecting the light from the carbon lamp[1] was turned so that its edge bisected vertically the photometric field at E, and the luminosity of the colored light was increased or decreased by adjusting the collimator slit until it equalled by the equality-of-brightness method the 12.5, 25 or 50 m.c. reflected from the disc D after the correction had been made for the absorption of lens B. The sensitivity of the determination by the equality-of-brightness method was approximately 2%, varying around this value with the color and intensity of the light. For the flicker determinations the disc D was rotated and the position of the carbon lamp adjusted on the bar until no flicker was obtained at the lowest possible speed that would give no flicker. At this speed the sensitivity of the determination was around 0.4%, varying with the color and intensity of the light.

In making the determinations for the rise of sensation curves for color, the disc D was turned completely out of the beam of light so that the entire field at E was seen as colored; and for the curves for white it was turned so as to cut off the colored light completely and reflect the light from the carbon lamp so that the entire field at E was seen as white (see footnote). The exposure disc F of the rotary tachistoscope was then turned into position. This disc was compounded from two pairs of discs having radii respectively of 17 and 24 cm. These discs were mounted on the axle of the tachistoscope, the position of which was so adjusted that the circular edge of the smaller pair of discs bisected vertically the field seen at E. These halves, right and left, of the photometric field formed the comparison and standard fields for the determination of the rise-of-sensation curves.

This determination was made as follows. The smaller set of discs was closed completely and the larger was opened by a series of adjustments until, with a given speed of the single rotation of the discs, a just noticeable

[1]For convenience this light will be designated as white in the rest of the paper and in the charts.

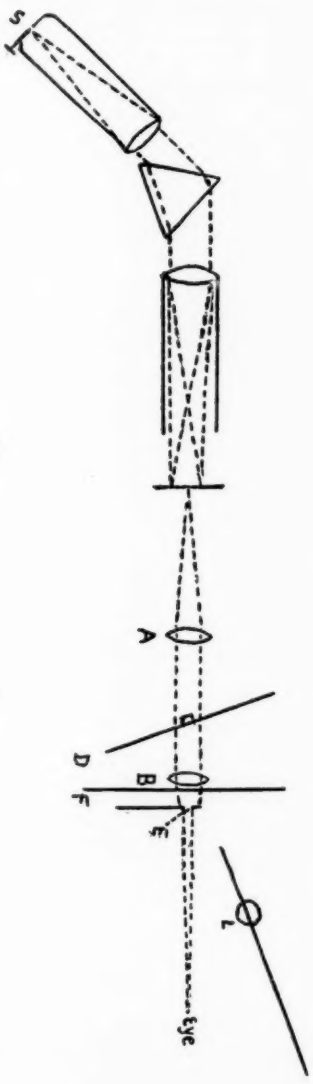


FIG. 1. Plan of Apparatus.

sensation was aroused. For the next observation the smaller pair of discs was given this value, and the opening of the larger pair was increased until with the same speed of rotation a just noticeable difference in brightness was obtained. The series was continued, the comparison opening for one observation being made the standard for the next, until a value of exposure was obtained, no further increase of which caused a noticeable increase of brightness of the comparison field. This was accepted as the optimum value of the sensation, and the sensations produced by other exposures were rated in terms of their number of just noticeable differences from zero. Further increase of length of exposure caused a decrease in the brightness of the comparison field, and a continuation of the series beyond this point gave the just noticeable difference values needed to plot the other half of the curve which represents the temporal course of sensation, rise and fall.

The exposure apparatus or rotary tachistoscope was made up of the rotation apparatus and two pairs of specially constructed sectored discs. The rotation apparatus consisted of a heavy Y-shaped stem with carefully turned bearings in which rotated a spindle fitted with an arbor and chuck to hold the exposure discs. Since only a single exposure was wanted for any observation, the discs were driven by a gravity or fall device. That is, on the other end of the spindle was the smaller of a two cog-wheel gear system. Attached at its middle to the larger cog-wheel was a rod 1 m. long designed to carry on its two arms equal weights. For most of this work, in order to give short exposures and high speeds, a heavy weight was carried on one arm alone. These weights, which slip on the rod, were of lead and were held in position by means of set-screws. The rod was graduated in mm. so that at any time the position of the weights could be read directly from the axle. Before an exposure the rod was held in position by a catch, the height of which was adjustable. When the catch was released the weight fell, rotating the discs with a velocity depending upon the height of the catch and the position of the two weights on the rod. After the fall the other end of the rod was caught automatically by a second catch.

Another factor in determining the length of the exposure is the displacement of the open sector from the path of the stimulus light or its position in the circle of rotation; for obviously upon this will depend, with a given height of the catch and a given position of the weights on the driving rod, the amount of acceleration which is given to the disc before the open sector reaches the path of the stimulus light. This value for the initial distance of the open sector from the path of the stimulus light was fixed by pinning permanently to the axle that disc of each set the edge of which made the beginning of the exposure, so that these edges were in the same radial line.

The values of the open sectors were measured with a Vernier protractor graduated to seconds of arc. These measurements after any number of sets of observations could be converted into units of time by a simple process of calibration. Smoked paper was clipped to the disc across the open sector; the pendulum was released with the weights, the starting point, etc., just as they were in the original observation; and a time line was run across the open sector by an electric tuning fork the vibration frequency of which was known. The paper was removed, shellaced, and counted at leisure.

Prior to the use of this method of determining the rate of rise of sensation, a preliminary study was conducted in our laboratory in which a comparison of results was made using all of the older methods and three new ones devised for the purpose.[2]

[2]For the details of this study, see M. A. Bills, The Lag of Visual Sensation in its Relation to Wave-length and Intensity of Light, *Psych. Monog.*, No. 127, 101 pp.

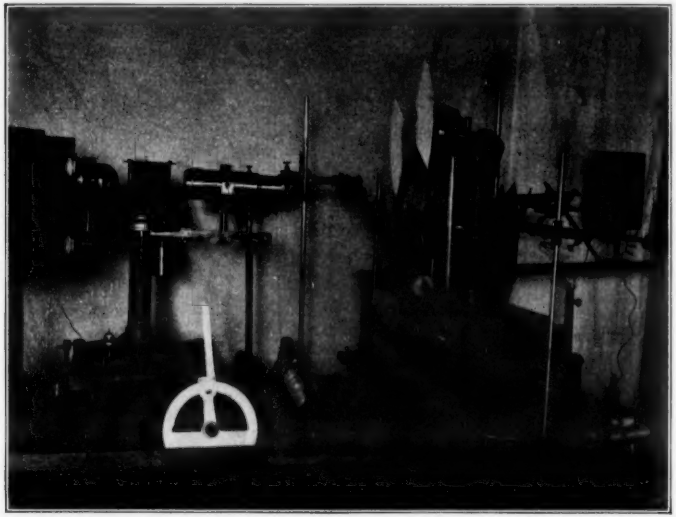


FIG. 2. Photograph of Apparatus.

The work was done with spectrum lights with the same *O* and state of adaptation of the eye and as nearly as possible the same apparatus for each of the methods. As the result of this intercomparison and interchecking of methods, the one used in the present study was selected as the most feasible and perhaps the surest in principle. The new methods, all of which were devised by the present writers, were designed not only to ascertain the time in which a given stimulus produces its maximum effect on sensation, but also to make possible a comparison of the sensation values at any point in the rise or fall. There are two possibilities of making accurate and reproducible judgments of sensation, namely, the judgments of equality and of just noticeable difference. Upon these judgments all quantitative work on sensory responses ultimately rests. Both of these were utilized in developing the new methods mentioned above. The development of these methods has, in fact, been just the systematic application of these two judgments to the problem in question in accord with the purpose mentioned earlier in the paragraph. The just noticeable difference obviously may be determined by holding the time of exposure constant and varying the intensity of light, or conversely by holding the intensity of light constant and varying the time of exposure. The latter principle was employed in the method used in this work. The sensation value obtained by this method may thus be called a time just noticeable difference.

In order to preserve as far as possible the same state of adaptation of the eye for the rise-of-sensation work as was used for the flicker and equality-of-brightness comparisons, the photometer lamp was always kept lighted and set to give the desired intensity. The period of adaptation to this light, the preexposure, etc., were carefully controlled.

The same *O*, one well trained in photometric and tachistoscopic observation, was used throughout the work reported in this and the following sections. It is to be regretted that the work of this first section is so laborious that it was impossible to repeat it on other *O*s. What was wanted here was a comparison of the flicker and equality-of-brightness ratings with the rise of sensation curves. We have as yet been unable to procure other *O*s sufficiently trained who could give the great amount of time needed for this work. In case of the supplementary data to be given in the next paper, however, the general trend of result has been checked and verified by other *O*s,—6 for the effect of change of intensity; 20 for the effect of speed; and one other (2 in all) for the agreement of the two photometric methods when the length of exposure is made equal. Results for more *O*s will be obtained on this latter point as soon as possible.

Curves representing the results obtained for the rise of sensation are shown in Charts 1-4. In these charts just noticeable

differences in luminosity are plotted along the vertical co-ordinate and time of exposure along the horizontal. Chart 1 shows the curves for the five lights at 12.5 m.c. At this intensity the curves were completed to the maximum and slightly beyond. As

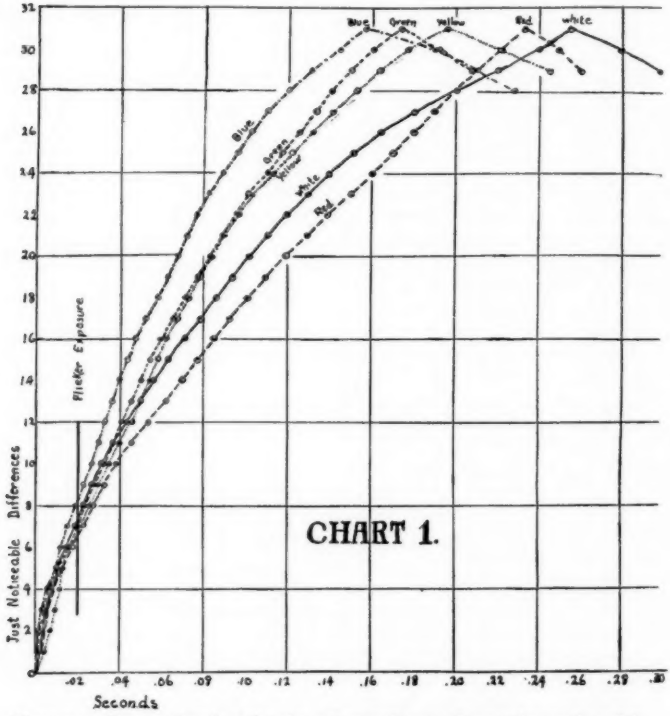
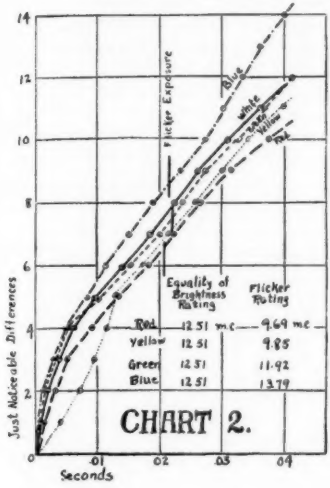
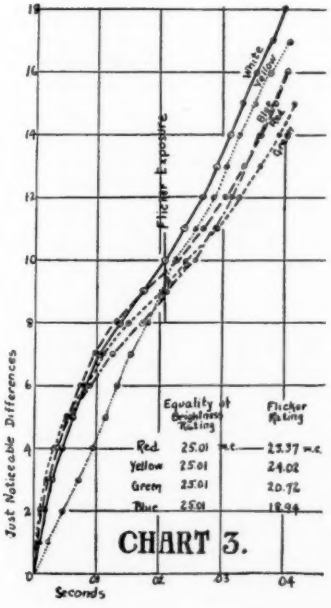


CHART 1. Curves showing the rise of sensation to the maximum for white, red, yellow, green and blue at 12.5 meter-candles

would be expected from the fact that all the lights were of the same photometric value, the same number of just noticeable differences (31) were obtained for each to the maximum. The optimum time of exposure for the blue was 0.157 sec.; for the green 0.174 sec.; for the yellow 0.195 sec.; for the red 0.233 sec.; and for the carbon lamp 0.254 sec.

In Chart 2 the lower part of the curves of Chart 1 is plotted on a larger scale. On these curves the height to which the sensation rises for a length of exposure equal to that of the





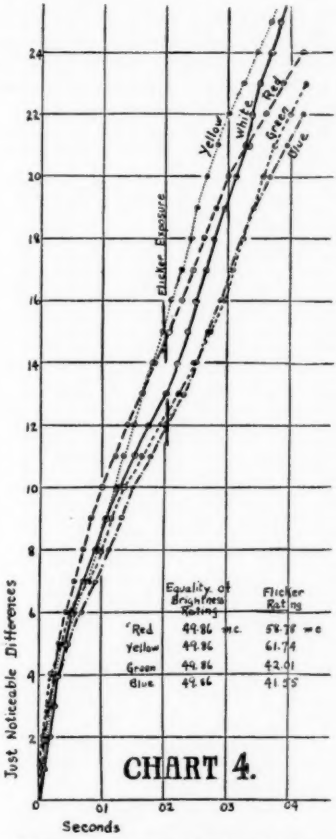


CHART 2. Curves showing the rise of sensation up to 0.04 sec. exposure for white, red, yellow, green and blue at 12.5 meter-candles

CHART 3. Curves showing the rise of sensation up to 0.04 sec. exposure for white, red, yellow, green and blue at 25 meter-candles

CHART 4. Curves showing the rise of sensation up to 0.04 sec. exposure for white, red, yellow, green and blue at 50 meter-candles

individual exposures used in the method of flicker is indicated by a short vertical line. In the use of the flicker method we worked at the lowest possible speed that would give no flicker. Since this speed is slightly different for the different colors, the values of the individual exposures were slightly different. On this chart are shown also the photometric values given to the colored lights both by the equality-of-brightness and the flicker methods. It will be noted that red, yellow and green are all underestimated by the method of flicker, red the most; and that blue is overestimated. An inspection of the curves shows that this result is as it should be according to the relative heights to which the sensations rise for the time of exposure used in method of flicker.

In Chart 3 curves are given for the rise at 25 m.c. These curves were not continued to the maximum because of the prohibitive amount of time and work required to make the complete series for lights of this intensity. The series was continued up to an exposure time of 0.04 sec. (15 to 18 just noticeable differences, depending upon the color), which is well in excess of the flicker exposure. The photometric results show that all the colors were underestimated in the order, from least to greatest, of yellow, red, green and blue. The curves for rise show that for the value of exposure used in the method of flicker, the carbon light has risen to the highest value in sensation,—then yellow, red, green and blue.

Chart 4 shows the curves at 50 m.c. These curves likewise were continued only up to 0.04 sec. (20 to 27 just noticeable differences). The photometric data show in order an overestimation of yellow and red by the method of flicker at this intensity and an underestimation of blue and green as would be expected from the curves in Chart 4. Several intersections in the curves will be noted,—one for red and yellow at an exposure slightly less than that used in the method of flicker, and one for green and blue at an exposure slightly longer than the flicker exposure.

In the next paper of the series curves will be given for each color and white at the three intensities. An exact correspondence in the type of deviation of the photometric results, and of their ranking as to amount with the respective differences in the heights to which the colored and white sensations have risen, will be noted.

It may be stated that the photometric and rise-of-sensation work was done in quite different series. The chance for bias by pre-knowledge of the results of one type of work before the other was done was negligible. For example, while the work was scattered through the years 1917-1922, the final conversion of the settings of the exposure apparatus into units of time was not made until late in the spring of 1922.

THE CONCEPT OF BACKWARD ASSOCIATION

By Hulsey Cason, Syracuse University

In discussing certain aspects of memory, various writers have referred favorably to the concept of backward association, a concept which is definitely traceable to Ebbinghaus' epoch-making work. We believe that this more or less uncritical acceptance of backward association—which is a subject of some theoretical importance—is a result of the natural difficulty which modern thinkers experience with the older laws of association, as well as of an improper interpretation of Ebbinghaus' work. In this paper, we shall attempt to point out some of the difficulties in accepting this concept. We shall not discuss the validity of the objective results themselves.

Ebbinghaus' classical study of memory showed that, when a series of nonsense syllables, 1, 2, 3, etc. is learned so that one correct reproduction is possible, associations are formed not only from syllable 1 to syllable 2, from 2 to 3, etc., but also from 1 to all of the other syllables in the series, the strength of the associations decreasing with the number of intervening syllables.[1] Other studies by Ebbinghaus also demonstrated the fact that associations are formed in the backward direction, as, for example, from 4 to 3, 4 to 2, etc.

Having obtained these reliable results, Ebbinghaus was somewhat perplexed concerning the theory of association which was current in his day. Apparently, while he was learning one association, still other associations were being formed at the same time. Why was even a weak connection formed between syllables 1 and 10, when in a single span of consciousness Ebbinghaus could grasp only seven syllables? And, most difficult of all to explain, why were *backward* associations formed when he seemed to be exercising the associations only in the *forward* direction? Ebbinghaus exclaimed: "How odd are the connections of human thoughts which jostle in their flight!" and in another place remarked that "the customary formulation, 'ideas become associated if they are experienced simultaneously or in immediate succession,' has something irrational about it."[2]

[1]The strength of the associations was measured by the familiar relearning method.

[2]H. Ebbinghaus, *Memory, a Contribution to Experimental Psychology*, 1885, 108 (trans. by H. A. Ruger and C. E. Bussenius). Throughout this discussion we shall refer only to Ebbinghaus' *Gedächtnis*, because it is in this work that his views on memory and association are most clearly stated. Neither in the *Grundzüge der Psychologie* nor in any other of Ebbinghaus' later works do we find a clear statement or a satisfactory explanation of backward association.

Ebbinghaus' own formulation of the law of association reads as follows: "Ideas which have been developed simultaneously or in immediate succession in the same mind mutually reproduce each other, and do this with greater ease in the direction of the original succession and with a certainty proportional to the frequency with which they were together."[3] This formulation agreed fairly well with Herbart's view, which seemed to Ebbinghaus to be a safe middle ground. In discussing Herbart's formulation, Ebbinghaus writes: "According to this conception, therefore, the associative threads, which hold together a remembered series, are spun not merely between each member and its immediate successor, but beyond intervening members to every member which stands to it in any close temporal relation. The strength of the threads varies with the distance of the members, but even the weaker of them must be considered as relatively of considerable significance. The acceptance or rejection of this conception is clearly of great importance for our view of the interconnection of mental events, of the richness and complexity of their groupings and organization. But it is clearly quite idle to contend about the matter if observation is limited to conscious mental life, to the registration of that which whirls around by chance on the surface of the sea of life."[4] Probably no one today would accept this general statement of the law of association. It appears that the principal difficulty is to be found in the nature of the learning process which Ebbinghaus employed, rather than in the objective results which he obtained. We may therefore discuss at this time the procedure which is commonly used in learning nonsense syllables.

No matter how expert one happens to be in concentrating on nonsense syllables, it is impossible to control the conditions in such a way that syllable 2 is thought of only after syllable 1, 3 only after 2, etc. While Ebbinghaus took practically all possible precautions to establish fairly constant experimental conditions, *some* fluctuations of attention did occur. In addition to thinking of 6 after 5, 10 after 9, etc., even a practised subject like Ebbinghaus found himself thinking occasionally of 8 after 4, of 12 after 3, etc.; and, doubtless to a lesser degree, also of 4 after 8, and of 3 after 12. The practice is not *all* from 1 to 2, 2 to 3, etc.; *some* practice takes place on other connections when these fluctuations of attention occur; and it is in these cases, where the learning process does not limit its activities to the associations which the subject is trying to form, that the additional associations, such as 4 to 8, 3 to 12, 1 to 3, etc., are formed. These considerations also apply to the learning of the "backward

[3] *Op. cit.*, 90.

[4] *Op. cit.*, 94.

associations:" practice is really forward in such cases, rather than backward. In learning the lists of syllables, there was also a certain amount of thinking about the *position* of certain syllables in the series, as, for example: "5 comes after 2 and 3," "7 and 8 come before 12 and 13," "I just repeated 8, " "here are two syllables which suggest the word so-and-so," etc. All of this constitutes *practice.*

It is significant that Ebbinghaus says very little about his *introspections,* which would have added to the value to his work. He was certainly not able to control his interest and attention *completely* for hourly periods, year after year, in such a burdensome and monotonous task as this. It will be remembered that Ebbinghaus attempted to learn each list so as to be able to repeat it just once correctly; and the relative time spent in attempting to reproduce the numerous lists was therefore considerable. During the unsuccessful attempts, especially, when he would think: "1, 2, 3, 4, 5, 6, 7, 8,. . . . . ?" it was only natural for him to think occasionally of 10, or 11, or 12, etc.

Certain experiments have shown that we frequently associate an item with a "whole," so that the item can later call up the whole, or *vice versa.* For example, the stimulus, town A, may cause one to think of one of a certain group of individuals whom he knew in this place. The nonsense syllables in Ebbinghaus' experiment were doubtless associated with the whole list in this same general fashion: Ebbinghaus would surely do *some* thinking about *this* list, in addition to jumping forward or backward occasionally to think about some nonsense syllable which particularly attracted his attention. When Ebbinghaus hesitated in repeating a series, pausing, for example, on syllable 10, it was only natural for him to hunt around (for the next syllable) in the *rest of the list,* recalling 12 or 13 or 14, etc., in this way forming a more definite association between 10, on the one hand, and 12 or 13 or 14, on the other.

The claim might be made that the formation of additional associations in this manner would be rather slight. This is doubtless true; but it is also true that when Ebbinghaus rearranged the lists (using 1, 3, 5, etc., or 2, 4, 6, etc.), the saving of work (in percent. of original learning time) was itself very slight, amounting to only 10.8%, 7.0%, 5.8% and 3.3% respectively when the number of intermediate syllables skipped in the formation of the derived series was 1, 2, 3 and 7.[5]

In another variation of the experiment, Ebbinghaus learned a list and then rearranged the nonsense syllables in a chance

[5]It should be added that these results, while of small magnitude, were quite reliable.

order (except that the first and last syllables remained the same as before); and he then relearned the list. There was little gain from the familiarity with the syllables: "the syllables were, therefore, in themselves, outside of their connection, so familiar to me that they did not become noticeably more familiar after being repeated 32 times."[6] With one group of tests he found a saving of only 12 sec. on a base of (roughly) 1261 sec. "In the case of the construction of a new series through a mere permutation of the syllables, there was an average saving of 12 seconds. . . . No evidence has been secured. . . . establishing the facilitation of the process of relearning a series by means of the identity of the syllables and the identity of the initial and final terms."[7] In another group of tests he found an average *increase* in expenditure of time of 5 sec. It seems therefore that the facilitating associations just about balanced the disturbing associations: so that there was little change in the efficiency of learning the two combinations. Some associations were now easier to form because of the slight practice, but other connections had to be unlearned. Ebbinghaus naturally became very familiar with the nonsense syllables which he used, and the learning process had to do principally with the formation of connections between different syllables.

It appears that Ebbinghaus' formulation of the law of association, as stated above, is entirely *too inclusive*. The actual practice of an association from 8 to 9 does not *itself* strengthen the association from 8 to 12 or from 8 to 4. During the practice, Ebbinghaus was exercising other bonds besides those from 1 to 2, 2 to 3, etc.; and some introspections would have indicated the nature of this practice much more precisely. The additional backward associations were formed in the same general fashion as the additional forward associations: the *practice* in these cases was simply "backward" instead of "forward." The later complete learning was of the same nature as the former incomplete learning had been. If practice had been confined entirely to the direction from A to B, then we feel sure that there would not have been the slightest tendency for stimulus B to call up response A.

In learning the backward associations, some practice was obtained during the regular learning of the series, as well as during the first imperfect recitation or recitations. The practice due to fluctuations of attention during the learning of the series seems to be more important in accounting for the backward associations. It is probable that highly trained subjects, by using a suitable exposure apparatus, could reduce this error

[6] *Op. cit.*, 106.
[7] *Op. cit.*, 101.

considerably. The error due to the first imperfect recitation or recitations might also be reduced to some extent, but it could probably not be eliminated completely. However, very careful introspections could throw some further light on the disturbing elements present, as well as upon the relative importance of these various factors.

There is a serious difficulty in regard to the physical mechanism involved in this so-called backward association, which is itself sufficient to discredit the assumption of this form of learning. In learning a series of nonsense syllables, the first visual stimulus is nonsense syllable 1, calling up by means of cells in the cortex a subvocal response of syllable 1. This motor response later becomes associated with nerve cells in the brain, which in turn cause the speech organs to pronounce (subvocally) syllable number 2, etc. The stimulus is visual at first, and the response is always motor. It would be impossible for such a specific chain of connections to operate backwards, partly because of the law of forward direction across the synapse. It is now generally recognized that it is misleading to speak of associating two *objects* in the mind. If we accept the view that an association is between a stimulus and a response, then it would be impossible for the association to function backwards. Although there is evidence of centrifugal sensory paths, the existence of these paths should probably not be taken as evidence favoring the possibility of backward association.

It is in general preferable not only to take account of the introspections of the subject, but also of the physical processes which are involved. The introspective evidence in this particular case would have identified the other factors at work in learning nonsense syllables, and a consideration of the physical factors involved, *i.e.*, the law of forward direction, etc., would have convinced workers in this field that the physical mechanism is highly improbable.

THE EFFECT OF GROUPING ON THE PERCEPTION OF DIGITS

By James D. Weinland

In a period of attentive vision so short as to preclude eye movements only a small number of impressions can stand out clearly, the span of attention being definitely limited. If we give but one glance at any unrelated collection of objects, as goods in a store window, items in a picture, or the like, we are able to enumerate only a few, probably but four or five of these objects. There are, however, occasions, in advertising, automobile license numbering, and many other phases of actual life, when for practical purposes as many objects or units must be presented simultaneously as can be clearly perceived. With a presentation, then, of a given number of units clear perception will depend on various factors of the exposure, highly important among which will be the spacing or arrangement of the units into groups. It is into this factor of grouping that the present investigation inquires. The experiment is limited to a study of rows of six digits presented in the different possible groupings.

For the scientific study of the area or range of visual attention it has been customary to employ some form of short exposure apparatus or tachistoscope, the principle of which is to furnish a fixation field which is supplanted for a measureable brief period by another field that contains the test material. There is thus a pre-exposure field, an exposure field, and a post-exposure field.[1]

The first gravity tachistoscope was devised by Wundt. Cattell constructed one of the early ones and used it in many of his experiments on reaction time and the attention span. It consisted of a screen which composed the pre- and post-exposure fields, the center being cut out so that as the screen fell the exposure field was presented to view. The screen was supported to slide as a window between guides at gravity speed; the exposure time was varied by changing the length of opening in the screen through which the exposure field was seen. Directly to the rear of the slide, the exposure field was mounted so that the screen would slide in front and in close proximity to it, exposing it as the cut-out portion of the screen slid past[2].

Wundt lays down the following rules for the proper use of the tachistoscope. (1) The exposure must be short enough to preclude eye movements. (2) The arrangement of the fixation mark and of the stimulus must be such that all constituents of the exposed objects can be seen with at least approximately equal distances; the exposure field must coincide with the ocular field of direct vision. (3) The exposure of all parts of the field

[1]G. M. Whipple, *Simpler Processes*, 266 ff.; R. Dodge, Psychology of Reading, *Psy. Rev.*, 8, 1901, 56; W. F. Dearborn, Perception and Reading, *Psy. Res. of J. M. Cattell*, 1914, 37.

[2]J. McK. Cattell, Ueber die Trägheit der Netzhaut und des Sehcentrums, *Phil. Stud.*, 3, 1886, 94ff.; G. M. Whipple, *Simpler Processes*, 262ff.

should be simultaneous, or so nearly so that there shall be no noticeable differences in time in the illumination of the various regions. (4) The retinal adaptation must be favorable, and sudden transition from dark to light must be avoided. (5) Persistent after-images must be avoided. (6) The duration of the retinal excitation must be limited enough to preclude roving of attention over the exposure field. (7) A ready signal must be given at an appropriate time before the exposure—Dodge adds three further suggestions. (1) The relative illumination of the pre-exposure, exposure, and post-exposure fields should be capable of experimental modification. (2) The exposure should be noiseless and free from distractions. (3) It should be possible to arrange for monocular or binocular observation.[3]

The previous work which most closely resembles the present investigation was done by Paul Ranschburg who, using the tachistoscopic method, studied the types and causes of errors in the perception of digits.[4] He had been examining a number of people on their range of perception when this problem interested him. He used 2, 3, 5 and 6 digit rows for his material, taking the digits from a handbook of statistics. He found that the perception threshold for heterogeneous rows was lower than for homogeneous rows of digits. When the rows contained identical elements the error score was the highest; it was somewhat smaller for similar elements, and least for heterogeneous rows.

identical elements	654536
Homogeneous rows:	
similar elements	926710
Heterogeneous rows	913845

Using arbitrary weighting devices he scored his results and found the percentage of error to be:

Rows with identical elements	87.7%
Rows with similar elements	76.4%
Rows with heterogeneous elements	38.5%

In the group of "similars" 3 was taken for 8, and conversely; 9 for 6; 2 for 0 or 9; and 1 for 7. In the heterogeneous rows the errors were principally transmutations, or reversals in the response of the order in which the digits were arranged in the stimulus: 90% of the total errors were made in the right half of the numbers, or from four to six inclusive in the series of six. In two-thirds of the cases errors were made in the next to the last digit, in nearly one-third of the cases the offending digit was the fourth, only seldom was it the last.

The experiment undertaken in the Psychological Laboratory at Columbia was somewhat similar to Ranschburg's. It differed, however, in seeking the effective grouping of digits to exclude errors rather than in examining the characteristic types of errors usually made.

The apparatus used consisted of the usual gravity tachistoscope modified by means of weights to control the speed of descent of the screen, and thus the exposure time. The weights operated in the following manner: a derrick was constructed to the rear of the tachistoscope and upon this derrick two small pulleys were mounted, one of the pulleys being directly over the screen, the other to the rear of the apparatus so that

[3] G. M. Whipple, *Simpler Processes*, 262ff.

[4] P. Ranschburg, Ueber Hemmung gleichzeitiger Reizwirkungen, *Zts. f. Psych.*, 30, 1902, 39ff.

a line running through it would suspend weights unobstructed by the apparatus. Through these pulleys a line was then run, to the falling screen at one end and to the weights at the other end. In this manner the falling screen was forced to pull a load as it fell, its speed being thus diminished. Weights were also attached to the screen itself, the object being to make the apparatus somewhat heavier in order that its operation might be more even, producing a more constant exposure time. By changing the weights a wide variation in speed of the falling screen and thus of the exposure time was attainable. By increasing the load the falling screen must pull, its speed was diminished; by decreasing the load it must pull, its speed was increased.

The speed finally chosen for the experiment was selected empirically. A subject attempted to read figures, similar to those used in the experiment, at different exposure times, the speed of screen-fall being varied by means of changing the attached weights as described. When the optimum speed was selected the weights were left constant throughout the entire experiment, and the exposure time was measured.

The time of exposure was measured with a Bergström chronoscope which had previously been checked against a Hipp chronoscope: 19 exposures were measured and their median time, after the probable error of the instruments was eliminated, was found to be 176 σ, with a P.E. of approximately 2 σ. This time of 176 σ was used throughout the experiment.

In regard to the time of exposure much controversy has taken place. The first arguments were for exposures as low as 1 σ, and this in spite of the fact that Dodge found the reaction time of the eye somewhere near 162-170 σ [5]. Dodge states, however, that exposures of such short periods as 1 σ are artificial, and give results removed from natural conditions, which provide no basis for general conclusions .[6] He recommends a time "well over 150 σ" for exposure where threshold values are not being specifically studied. Huey finds that the fixation pauses in reading average about 185 σ, for fast readers, but with a large variation.[7] The exposure time used here is thus "well over 150 σ", approximating the average reading fixation, and providing a normal stimulation.

The tachistoscope screen used in this experiment consisted of a black pre-exposure field, an opening for the exposure, and a post-exposure field. It was made to slide in close proximity before the figures to be exposed so that the eyes should be properly focused for the figures, during the exposure time. Both pre- and post-exposure fields were black, though upon the pre-exposure field a small white line was placed upon which the subject could fixate his eyes as the screen fell, in order that his eyes should be properly focused for the figures.

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[5] R. Dodge, The Reaction Time of the Eye, *Psy. Rev.*, 6, 1899, 477ff.
[6] R. Dodge, Study of Visual Fixation, *Psy. Rev. Mon. Sup.*, 1907, No. 35.
[7] E. B. Huey, *Psy. and Ped. of Reading*, 1908, 56ff.

The essentials laid down by Wundt for tachistoscopic work, which we mentioned above, were all complied with in the apparatus used for this experiment; though as relative and not absolute phenomena were studied the added suggestions of Dodge as to lighting and binocular *versus* monocular vision were not carried out. There was some noise in the apparatus used, principally a thud as the screen reached the end of its fall; but the sound was slight, and no subject in his introspections mentioned it as a distraction.

The digits and the order of their presentation throughout the experiment were determined by chance, as follows. All the digits from 0 to 9 were written on small paste-board cards of equal size and shape, placed in a box, and thoroughly shaken up. They were then strewn upon the table, and the six nearest to *E* were picked up in the order in which they fell upon the table and were used for a group. It will be observed that, with only one each of the 10 digits used in the shuffle, repetitions in the group of six selected digits were impossible and so did not occur,—save in one instance in the series of 100 groups when a 4 was repeated by typographical error within a single group.

The arrangement of the groups then followed. In all of the groups there were six digits; but while in some groups the six were all equally spaced (271056), in others the six digits were broken into smaller units; 3-3, 2-2-2, 4-2 and 2-4 were used, as for instance: 194 053, 17 59 84, 8572 34 and 40 9516. There were 5 of these groups, and in the experiment there were 20 repetitions of the group-form exposure. Identical group forms were not composed of identical digits; the digits, as previously explained, were selected by chance for all the 100 exposures alike.

The occurrence of the groups in the exposure series was also determined by chance in the same way that the digits were originally selected. Five digits, there being five groups, were written on paste-board cards of equal size and shape and shaken up in a box. They were then thrown upon the table and taken in the order of their fall to represent the order of exposure of the groups in the experiment. The cards with the 5 digits were then again shuffled and thrown, the resulting order determining the order of the next 5 groups of digits. This procedure was repeated till the order of grouping of the whole series of 100 was arranged.

With the digits and the order of group presentation determined, the next step taken was to type these digits upon a long roll of paper at intervals of 14 mm., the beginning of each group the width of 6 spaces (typewriter) from the edge of the roll of paper. This roll was then used as an exposure series, and was slipped in and out through two slits in the back of the exposure

field, the side presenting the numbers being turned toward the subject, so that as the screen fell the numbers should be visible for the exposure period. Before the experiment the number series was run through the tachistoscope, and with each group in the center of the exposure field a mark was placed upon the back of the paper, in order that later *E* could make sure that each number series would be in the center of the field of vision during the tachistoscopic exposure.

A short practice series of digit groups was arranged for use in the experiment, composed and presented in the same manner as the experimental series; 9 groupings were used in this series.

Procedure

The subject was seated before the tachistoscope at a distance from the screen which he selected during the practice series, and which varied with the different subjects from 2 to 4 ft. Paper and pencil were given him to write down his observations. His instructions were as follows: "Look at the fixation point before you. A series of groups of digits will be shown you which you are to repeat. In each exposure there will be six digits; be sure and write them all. If you are not sure of any digit, guess."

E, after presenting the practice series, then inserted the experimental list of digits into the apparatus as previously described. The roll was pulled to the position already marked as having the first group of digits in the center of the exposure field, the subject was given the warning signal, and the catch releasing the screen was released. While the subject was recording his observation, the screen was raised to its original position, the series of digits was advanced till the next group was in the center of the exposure field, and all was ready for a second exposure. This procedure was repeated till the entire series had been shown. No attempt was made to equate the inter-exposure times, the only caution taken being to avoid hurrying the subject.

The observations of the subject were compared with the series exposed. It was found necessary to weight the results in order to score them, and the following method was taken. The weighting was by means of penalties. For all 6 digits correct the subject was given a score of 6. For each omission or error he was penalized 2 points, for a digit added he was penalized 1 point, and for each transmutation he was penalized 1 point. If 2 numbers were transmuted together, as 3-6 for 6-3, just 1 point was taken for this mistake. As the results sought were comparative, and this system of weighting applied to all the group alike, its use could not bias the results.

Ten subjects took part in this experiment; 8 were men, and 2 were women. All were graduate students in the department

of Psychology. The two who made the highest scores are mathematicians, and it seems that their accustomed use of figures aided them. Nearly all the subjects had previously performed a laboratory experiment testing out Ranschburg's findings, and so were familiar with the general type of the experiment.

Following is a tabulation of the scores. After the actual scores were obtained they were turned into percentages of possible scores. A total score of 120 was possible for each group, and so a score of 93 would mean a percentage score of 77.5. These percentage scores were added, compared, and their probable errors calculated. Thorndike's table was then referred to, to find the chances of there being an actual difference between them.[8] In the tables the different subjects will be designated by the letters: A B C D E F G H I and J. In the graph the averaged results from all the individuals are shown.

TABLE A. SCORES

	6 digits in groups of 2	6 digits in a single group	1st group 2 digits; 2nd group 4	1st group 4 digits; 2nd group 2	2 groups of 3 digits each
Subject					
A	.558%	.683%	.566%	.625%	558%
B	.708	.766	.600	.735	.658
C	.808	.616	.633	.658	.625
D	1.000	.941	.975	.958	.933
E	.658	.775	.758	.783	.608
F	.916	.833	.791	.775	.783
G	.641	.700	.716	.633	.473
H	.741	.616	.675	.700	.733
I	.725	.800	.666	.616	.683
J	.450	.408	.425	.283	.308
Av.	.7205	.7138	.6805	.6756	.6364

TABLE B. RELIABILITY

Averages	S.D.	P.E.	PEA	A-A	x/Q[9]	Table[10]
.7205	4.01	2.70	.85	.0		
.7138	4.39	2.96	.93	.67	.53	6396
.6805	4.37	3.14	.93	4.00	3.12	9823
.6756	5.16	3.48	1.10	4.49	3.23	9852
.6364	5.12	3.45	1.09	8.41	6.08	certain

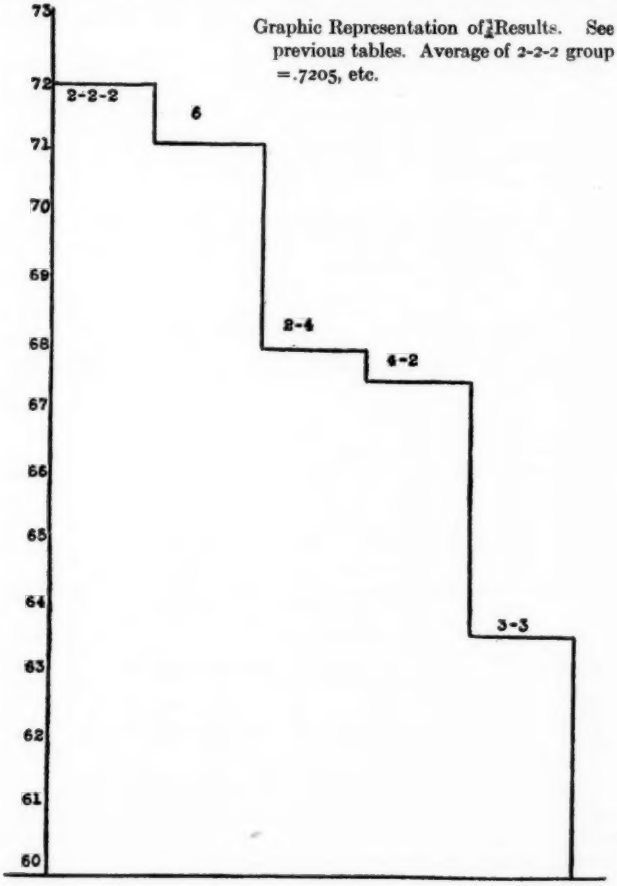
It is evident from the tables given above that the 6 and 2-2-2 groupings are approximately the same, the 2-2-2 group leading but by a margin that is hardly reliable. Both of these groups, however, are better than any of the others.

[8] E. L. Thorndike, *Mental and Social Measurements*, 1913, 200.

[9] $x/Q = \frac{\text{A-A}}{\text{PE of Diff.}}$

[10] Table = Thorndike, chances in 10,000 of a difference: see *Mental and Social Measurements*, 200.

No explanations seem necessary of these findings, save that they are facts. To theorize about them one might say that habit possibly helped in the perceiving of the 2-2-2 group, as



Graphic Representation of Results. See previous tables. Average of 2-2-2 group = .7205, etc.

much of our daily financial dealing is with two-figure groups. That the six-group was better than the others may have been because it was a unit; and that it fared worse than the 2-2-2 group, because it was too large a unit. There was no indication

given in the introspections just why any of the other groups should have been difficult. It may have been because attention was divided. Perhaps one part of the group was overperceived, owing to the separation, with a consequent failure in the other part. These are but possible explanations, however, the facts remaining that the 6 and 2-2-2 groups gave the best results.

Following is a table showing the distribution of the errors throughout the different groups. The letters T, E, O, A stand for transmutations, errors, omissions, and additions of digits by the various subjects during the experiment.

TABLE OF ERRORS

Grouping	2-2-2	6	2-4	4-2	3-3	Total
T	57	73	75	64	64	333
E	83	65	80	100	95	423
O	18	19	23	18	27	105
A	4	3	5	4	1	17
Totals	162	160	183	186	187	878

It will be observed from the above table that 90 more groups had errrors than transmutations, the type of mistake which followed next in frequency. The 2-2-2 group had a comparatively large proportion of errors, while the 6 row had the fewest groups with errors, but had a high percentage of transmutations.

This table compares favorably with the general findings of the experiment, though it indicates only the number of groups in which the different types of errors were found, and does not take into consideration the number of errors within the groups or their weighting. It will be seen, however, that by this table the 6 and 2-2-2 groupings are comparable in number of total mistakes, with considerably less than the other groupings.

Conclusion

The results show a scarcely dependable difference between the perceptions in the 6 and 2-2-2 groups, but either of these groups is definitely better than any of the other groupings used in this experiment for the perception of 6 digits, the 3-3 group being definitely the worst.

WEBER'S LAW AS TESTED BY FLOWING INCREMENTS

By A. D. Bush and Alice May Austin

Weber's Law has been examined so often that a further presentation would not seem to be justified unless some new method of testing were utilized. As is well known, the usual method of investigating the validity of this law for pressure has been by using definite multiples and fractions of the initial weight as succeeding stimuli for evoking the secondary sensation. An approach to our method was suggested by Witmer, who proposed using flowing sand; but flowing sand is not easily controllable in small increments, or in rapid variation of rate of increment. So we devised the simple method of using water as the variant for measuring definite appreciations of value, both of pressure and of tension increments.

Method. The apparatus used consisted of a glass cylinder (or burette) 152 cm. long and 2 cm. wide, serving as a head reservoir; and a conducting rubber tube, 2 mm. wide, running from the lower end of the reservoir through a stopcock down into beakers of varying weight. The stopcock could be so adjusted that the flow of water into the beaker was at the rate of 0.5 cc. per sec., or at the rate of 1.33 cc. per sec., or at the rate of 3.33 cc. per sec., or at the rate of 7.33 cc. per sec. The burette was connected with a second reloading reservoir so that the initial head in the service burette could be reestablished before each test. Three beakers were used for the successive tests, each so loaded as to give initial pressures of 100 gm., 150 gm., and 300 gm., respectively; being thus in the ratio of 1:1.5:3.

Two sets of experiments were carried out, one testing appreciation of simple varying pressures, the other testing appreciation of varying pressures when there was also involved the factor of steadily-increasing tendon tension; that is, in the first test, the beaker rested on the subject's hand (a thin layer of cotton intervening), and the hand rested on a cotton-padded board; in the second test, the hand holding the beaker was supported from the wrist only which, in turn, rested on the cotton-padded board.

The subjects were 15 students from the classes in Medical Physiology at the University of North Dakota. With each subject three readings were taken for each of the four rates for each of the three beakers for each sense—pressure and tension.

Results. The final average results are tabulated as follows.

TABLE I

Pressure Averages

	Beaker 1 Initial Weight 100 gm.	Beaker 2 Initial Weight 150 gm.	Beaker 3 Initial Weight 300 gm.
Rate I = 0.5 cc./sec.	Increment 14.78 gm.	Increment 24.24 gm.	Increment 36.83 gm.
Rate II = 1.33 cc./sec.	26.63 gm.	36.65 gm.	44.78 gm.
Rate III = 3.33 cc./sec.	36.98 gm.	47.96 gm.	73.60 gm.
Rate IV = 7.33 cc./sec.	46.98 gm.	63.34 gm.	100.5 gm.

The percentage increment in each case is as follows.

	Beaker 1	Beaker 2	Beaker 3
Rate I	14.78%	16.16%	12.28%
Rate II	26.63 "	24.43 "	14.93 "
Rate III	36.98 "	31.97 "	24.53 "
Rate IV	46.98 "	42.23 "	33.5 "

These findings seem not to be in conformity with either Weber's Law or Fechner's formula.

TABLE II

Average of Muscle Tensions

	Beaker 1 Initial Weight 100 gm.	Beaker 2 Initial Weight 150 gm.	Beaker 3 Initial Weight 300 gm.
Rate I = 0.5 cc/.sec.	Increment 11.64 gm.	Increment 18.15 gm.	Increment 26.71 gm.
Rate II = 1.33 cc./sec.	16.50 gm.	22.87 gm.	33.97 gm.
Rate III = 3.33 cc./sec.	32.89 gm.	48.24 gm.	64.14 gm.
Rate IV = 7.33 cc./sec.	41.50 gm.	57.65 gm.	73.95 gm.

The percentage increment in each case is as follows.

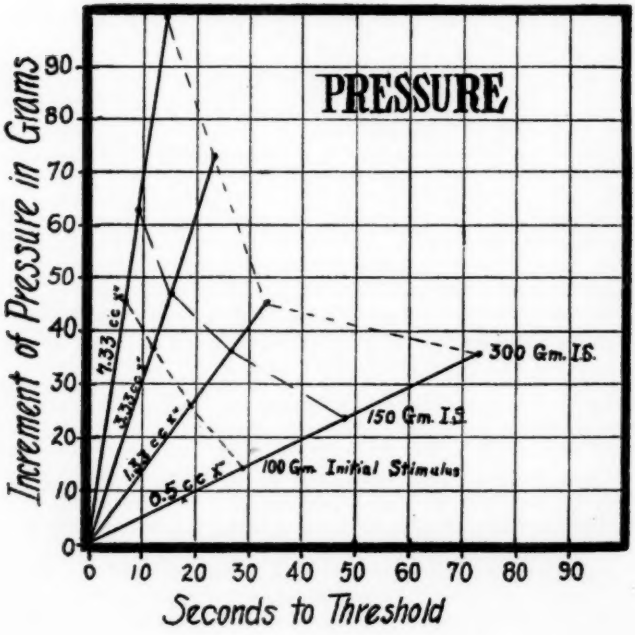
	Beaker 1	Beaker 2	Beaker 3
Rate I	11.64%	12.10%	8.9%
Rate II	16.5 "	15.25 "	11.36"
Rate III	32.89 "	32.16 "	21.38"
Rate IV	41.5 "	38.43 "	24.63 "

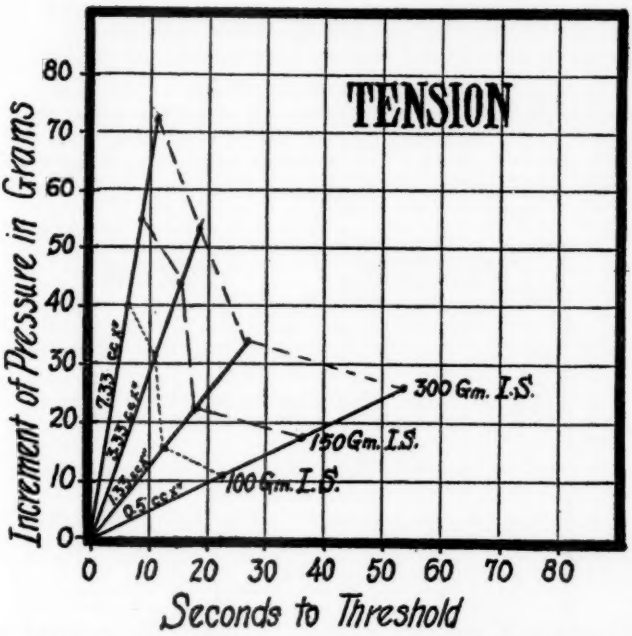
These findings, also, seem not be to in conformity with either Weber's Law or Fechners' formula.

The two following graphs indicate perhaps even more clearly that, in these experiments, the law of Weber did not obtain.

Perhaps one of the most important findings resulting from this investigation is the evidence that the psychophysical reaction is to a measurable degre einfluenced by the gm.-sec. value

of the stimulus. With an initial weight of 100 gm. it required 29.5 sec. for appreciation of an increment of 14.78 gm. if added at the rate of 0.5 gm. per sec., whereas it required a little over 6 sec. for appreciation of an increment of 46.98 gm. if added at the rate of 7.33 gm. per sec., a little over 11 sec. for appreciation of an increment of 36.98 gm. if added at the rate of 3.33 gm. per sec., and a little less than 18 sec. for appreciation of 26.33 gm. if added at the rate of 1.33 gm. per sec. Expressed in gm.-sec. the statement in the preceding statement appears thus: 436.89 gm.-sec., 300.67 gm.-sec., 399.47 gm.-sec., 479.34 gm.-sec. That is, when the initial stimulus was 100 gm., the psychophysical threshold was crossed most quickly when the increment was at the rate of 7.33 gm. per sec., and was most delayed when the increment was at the rate of 1.33 gm. per sec. When the initial stimulus was 150 gm., the threshold was crossed most quickly when the rate was 7.33 gm. per sec. (total equals 544.3 gm.-sec.), and most slowly when the rate was at 0.5 gm. per sec. (total equals 1175.15 gm.-sec.) When the initial stimulus was 300 gm., the threshold was crossed most





quickly when the rate was 7.33 gm. per sec. (total equals 1315.5 gm.-sec.), and most slowly when the rate was 0.5 gm. per sec. (total equals 2712.8 gr.-sec.).

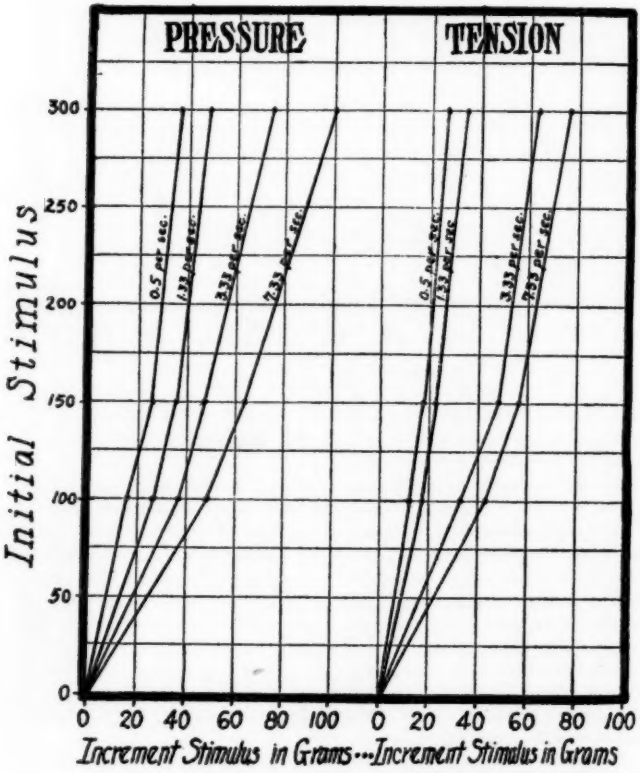
In the series of experiments testing tension, when the initial stimulus was 100 gm., the threshold was crossed most quickly when the rate was at 1.33 gm. per sec. (total equals 198 gm.-sec.), and most slowly when the rate was 3.33 gm. per sec. (total equals 328.9 gm.-sec.); when the initial stimulus was 150 gm., the threshold was crossed most quickly when the rate was at 1.33 gm. per sec. (total equals 381.4 gm.-sec.) and most slowly when the rate was at 3.33 gm. per sec. (total equals 699.6 gm.-sec.); when the initial stimulus was 300 gm., the threshold was crossed most quickly when the rate was at 7.33 gm. per sec. (total equals 739.5 gm.-sec.), and most slowly when the rate was at 0.5 gm. per sec. (total equals 1476.4 gm.-sec.)[1].

That the results obtained by the experiments with water increments are not in accord with either Weber's Law or Fech-

[1]It will be understood that this threshold discussion refers not to elapsed time but to gm.-sec. time.

ner's formula is shown graphically in the two appended curves, each of which summarizes the accrued data; probably they are sufficiently self-explanatory to require no further elucidation.

Summary. A series of experiments wherein Weber's Law and Fechner's formula are tested by means of flowing water increments for the secondary stimuli fail to show conformity to either law or formula. Curves presented show this non-conformity graphically.



EFFECT OF LENGTH OF LIST UPON MEMORY FOR NUMBERS[1]

By EDWARD S. ROBINSON and CHESTER W. DARROW, University of Chicago

PAGE
I. Problems and Methods 235
II. Length and the Curve of Memorizing 237
III. Length and Difficulty 239
IV. Length and Amount Retained 240
V. Length and Susceptibility to Retroactive Inhibition 241
VI. Summary of Results 243

I. Problems and Methods

One method of testing the reliability of experimental results consists in the application of certain statistical inferences based upon the number of observations involved and the dispersion of the quantitative results yielded by those observations. A method which is somewhat simpler than, but by no means exclusive of, the first consists in a repetition of the experiment under the same general conditions and somewhat different detailed conditions. Results that appear in two or more experiments of the same general type not only secure a certain statistical guarantee but also an increase in the generality of their significance. The present experiment is an application of the test of repetition to the results of an earlier experiment conducted in the Chicago laboratory.[2] This second experiment is by no means a complete duplication of the first. If it were, its agreement with the first would scarcely add anything to the implication of their common results.

Each experiment made a simultaneous attack upon four different problems: (1) the relation between the length of material memorized and the form of the curve of memorizing; (2) the relation between length of material and the effort required to memorize it; (3) the relation between length and retention; and (4) the relation between length and susceptibility to retroactive inhibition.

In the first experiment the materials memorized were lists containing 6, 9, 12, 15, or 18 nonsense syllables. In the second experiment the materials memorized were lists of 4, 6, 8, or 10 three-place numbers. In both experiments the exposure time for the single member of a list was 2 sec. In both experiments the anticipation method was employed. That is, after the first

[1]Read in part before the American Psychological Association, December 27, 1923.

[2]E. S. Robinson and W. T. Heron, *J. of Exper. Psychol.*, 5, 1922, 428-448.

presentation of a list, the subject, upon the presentation of any syllable or number, tried to anticipate the next syllable or number in the list. This procedure was employed both in the original learning of a list and in the relearning of it. Two scores were obtained:[3]

$$\text{Recall Score} = \frac{\text{Number of numbers correctly anticipated during first re-presentation of list}}{\text{Number of numbers in list}-1^{4}} \times 100$$

$$\text{Saving Score} = 100 - \left\{ \frac{\text{Number of presentations required on relearning}}{\text{Number of presentations required for original learning}} \times 100 \right\}$$

The time between learning and relearning was 20 min. in Experiment 1 and 15 min. in Experiment 2. This period was devoted either to the memorizing of a second list of syllables or numbers, or to casual reading of the daily paper. Different groups of 10 subjects each were used in the two experiments. In both cases practice effects were reduced, though not eliminated, by 6 preliminary sittings. In Experiment 1 each subject went through all of the conditions 3 times; in Experiment 2 each subject went through all of the conditions 4 times. Of course, the conditions were gone through in varying order by the different subjects. In neither experiment did a subject work under more than one condition on a single day.

Table I displays the plan of our second experiment.[5]

TABLE I

O = Original memorizing of a list of three-place numbers, figures 4, 6, 8, and 10 designating the number of numbers in the list.
I = Memorizing a list of three-place numbers immediately following the memorizing of a first or original list. The figure 6 designates the number of numbers in this interpolated list.
Rest = Casual reading of the daily paper with no attempt to memorize the material read. During this period the subject refrained from rehearsing O or I.
RO = Relearning of original list of numbers. The interval between the completion of O and the beginning of RO was always 15 minutes.

Condition		Interpolated Period of 15 min.	
I	O 4	I 6, Rest	RO
II	O 4	Rest	RO
III	O 6	I 6, Rest	RO
IV	O 6	Rest	RO
V	O 8	I 6, Rest	RO
VI	O 8	Rest	RO
VII	O 10	I 6, Rest	RO
VIII	O 10	Rest	RO

[3]Compare *op. cit.*, 429-430.
[4]The first number was used only as a cue. The subject was not expected to recite it while the last number was before him.
[5]For a tabular plan of the experiment with nonsense syllables, see earlier paper, 431.

II. Length of Material and the Curve of Memorizing

In Figure 1 we have plotted absolute amounts of material recalled against absolute amounts of learning time for the various number lists of Experiment 2. Learning time was measured in terms of the total number of numbers (not necessarily different numbers) presented. Thus two presentations of a 4-number list were considered equal to one presentation of an 8-number list. The curves represent the experiment as a whole. Each curve

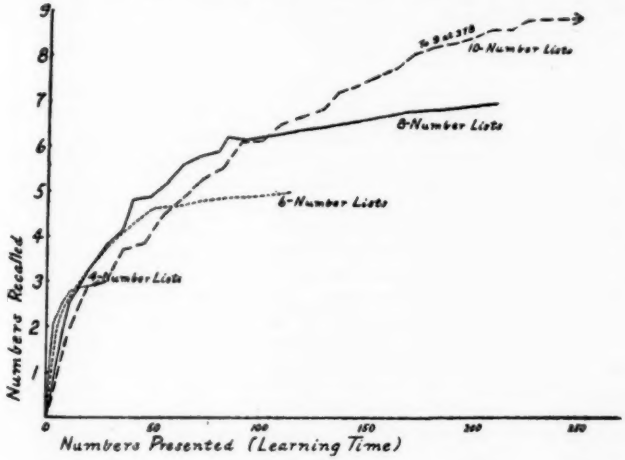


FIGURE 1

therefore is a composite of 80 individual curves. The curves for materials of different length all take the negatively accelerated form frequent among learning curves of most varieties. In this they agree with the curves obtained in Experiment 1, where nonsense syllables were memorized instead of numbers.[6] Perhaps a more important fact is the regular variation of the curves with variation in the length of material. The shorter the list, within the limits of our experiment, the more rapidly learning progresses during its early stages. The presentation of a list of 4 numbers twice, for example, results in the learning of more numbers than does the presentation of a list of 8 numbers once. This finding also is in exact accord with the experiment we were seeking to test. There are at least two possible explanations for the inverse relationship of initial learning rate to length. While the longer lists offer more possible associations, they also offer

[6] *Op. cit.*, 433.

more chances for interference effects, and their presentation during any absolute period of time must of necessity contain a smaller element of repetitiveness than the shorter lists.

Kjerstad concluded from the results of certain experiments of his own[7] that the form of the curve of memorizing is independent of the length of the material memorized. He held this to be true not when absolute study time and absolute amount learned are considered, but rather when the curve for material of a given length is plotted on the basis of the fractions of that material memorized in successive fractions of the total time required to master that material.[8]

In our earlier paper we converted the curves derived by plotting absolute amounts learned in absolute units of learning time into relative curves. That is, we found what fractions of material of a given length were learned in certain selected fractions of the time required to learn material of that length.[9] Our results confirmed those of Kjerstadt. Our method of transforming the absolute curves into relative curves was, however, somewhat different from that of Kjerstadt. We first obtained absolute memory curves from the pooled data of our entire experiment and then converted those curves directly into curves of the relative type. Kjerstadt, on the other hand, transformed the data obtained from the performance of each individual with each list into a curve of the Vincent type. Since the publication of our first paper we have decided that Kjerstadt's method is preferable, principally because the unusual performance and the unusual individual have less effect upon the final result. The total time required to learn a list was, in terms of our previous procedure, the time required for the slowest performance of the slowest learner. But by converting each curve obtained from each individual's learning of each list into a curve of the Vincent type, as Kjerstadt did, and then pooling the 80 Vincent curves thus secured, we obtained for the experiment with three-place numbers the facts shown in Table II.

A study of Table II shows the Vincent curves to be practically identical for lists containing 6, 8, or 10 three-place numbers. So far our findings are in accord with those of Kjerstadt. The curve for lists containing 4 numbers does not, however, conform to the curves for the longer lists. In this case relatively less is learned in the first fractions of the total learning time.

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[7] *Psychol. Monog.*, 1919, 26.

[8] Vincent (*Behav. Monog.*, 1912, 1, 15-17) seems to have been the first to give learning data this particular form of statistical treatment. This type of learning curve has properly been spoken of as the Vincent curve.

[9] *Op. cit.*, 435-436.

TABLE II

Percentages of Lists of Different Lengths Learned in Selected Percentages (sixths) of the Total Time Spent in Learning Those Lists

Percentages of Total Learning Time	Length of Lists			
	4 numbers	6 numbers	8 numbers	10 numbers
16.7	19.4	31.8	29.3	30.4
33.3	36.1	44.5	45.9	45.3
50.0	49.8	54.0	55.3	54.6
66.7	63.1	62.7	64.0	64.7
83.3	82.3	71.0	75.8	70.4
100.0	100.0	100.0	100.0	100.0

Since, as we have already said, our method of obtaining the relative curves of memorizing for the nonsense syllables employed in our first experiment was somewhat questionable, we have constructed Vincent curves for memorizing nonsense syllables by means of the method employed for the number lists. The values obtained are included in Table III.

TABLE III

Percentages of Nonsense Syllable Lists Learned in Selected Percentages (sixths) of the Total Time Spent in Learning Those Lists

Percentages of Total Learning Time	Length of Lists				
	6 syllables	9 syllables	12 syllables	15 syllables	18 syllables
16.7	23.3	33.9	34.6	30.9	33.1
33.3	45.0	51.2	52.3	52.2	53.2
50.0	60.5	62.0	66.3	62.0	66.3
66.7	72.3	73.6	74.9	74.2	76.3
83.3	83.4	80.8	80.5	80.3	82.8
100.0	100.0	100.0	100.0	100.0	100.0

The relative curves for 9-, 12-, 15-, and 18-syllable lists are practically coincident, but as in the case of three-place numbers there is evidently a lower limit to this law. The curve for 6-syllable lists, like that for the 4-number lists, shows smaller fractions of the total material learned in the first fractions of the total learning time.

III. Length and Difficulty

Figure 2 shows the relationship between the number of numbers in a list and the absolute time required to learn it. The four curves represent different cycles of the experiment. Here again the results are in agreement with those obtained for

nonsense syllables. With increasing length, the time required for learning increases at an increasing rate. The data available from experiments by Binet and Henri, Ebbinghaus, Henmon, Lyon, and Meumann show, with certain minor exceptions, the same increase in difficulty with increasing length.[10] It is true that the relationship between length and difficulty was once

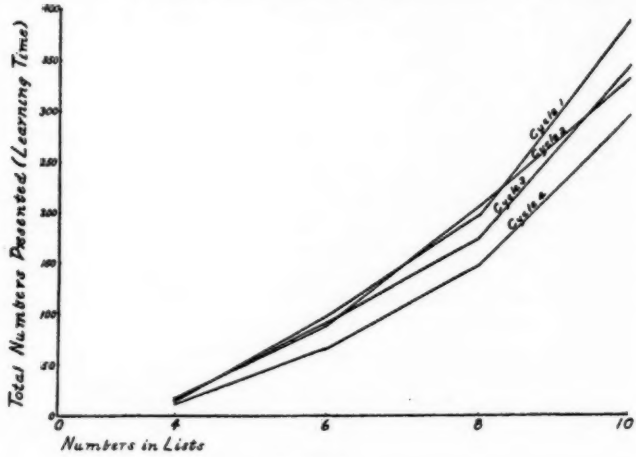

FIGURE 2

debated; but that debate was made possible only because of the procedure adopted by some investigators of discussing the relationship between length and repetitions. Since the repetition is a unit whose value changes with changes in the length of the lists, we may well abstain from dealing with it in this connection. Psychological statistics have horrors enough when they deal with constant units.

IV. Length and Amount Retained

Our experiment with nonsense syllables agreed with the general run of previous work in showing relatively slower forgetting for the longer lists. Table IV contains the percentages of lists of different lengths *recalled* after 15 min. of newspaper reading. Table V contains similar percentages in terms of *saving*.

[10] *Op. cit.*, 436-441.

TABLE IV

Percentages *Recalled* After 15 min.
$n = 10$ for each value except averages, where $n = 40$

Length of List	4 numbers	6 numbers	8 numbers	10 numbers
Cycle 1	46.6	66.0	68.5	74.4
Cycle 2	66.6	58.0	64.2	61.1
Cycle 3	50.0	72.0	56.6	71.1
Cycle 4	76.7	70.0	77.2	76.7
Av. of 4 cycles	60.0	66.5	66.6	70.8

TABLE V

Percentages *Saved* After 15 min.
$n = 10$ for each value except averages, where $n = 40$

Length of List	4 numbers	6 numbers	8 numbers	10 numbers
Cycle 1	−8.6	63.7	58.9	83.9
Cycle 2	56.0	64.9	73.7	74.9
Cycle 3	36.6	86.7	70.0	81.6
Cycle 4	16.0	74.9	75.0	75.3
Av. of 4 cycles	25.0	72.5	69.4	78.9

The clearest tendency, as in the case of nonsense syllables, is in the direction of relatively more forgetting for shorter than for longer lists. This tendency is decidedly more evident according to the *saving* criterion than it is according to *recall.*

V. Length and Susceptibility to Retroactive Inhibition

The reader will remember that the period between learning a list and relearning the same list was sometimes devoted to casual newspaper reading and sometimes to learning another list; that other list, as previously stated, always contained 6 numbers. Following the usual custom we shall call newspaper reading the *rest* condition and learning another list the *work* condition. Now the absolute amount of retroactive inhibition is equal to the

TABLE VI

Differences between Percentages of Different Lists *Recalled* Under *Rest* and *Work* Conditions: Absolute Inhibition
$n = 10$ for each value except averages, where $n = 40$

Length of Lists	4 numbers	6 numbers	8 numbers	10 numbers
Cycle 1	36.6	48.0	28.5	26.7
Cycle 2	33.3	44.0	34.2	7.8
Cycle 3	10.0	42.0	36.6	27.8
Cycle 4	43.4	46.0	37.3	33.3
Av. of 4 cycles	30.8	45.0	34.1	23.9
Percentages above Differences are of Amounts *Recalled* under *Rest* Condition: Relative Inhibition				
Cycle 1	78.5	72.7	41.6	35.9
Cycle 2	50.0	75.9	53.3	12.8
Cycle 3	20.0	58.3	64.7	39.1
Cycle 4	56.6	65.7	48.2	43.4
Av. of 4 cycles	51.3	68.1	52.0	32.8

TABLE VII

Differences between Percentages of Different Lists *Saved* under *Rest* and *Work* Conditions: Absolute Inhibition
$n = 10$ for each value except averages, where $n = 40$

Length of Lists	4 numbers	6 numbers	8 numbers	10 numbers
Cycle 1	5.3	34.5	4.7	22.8
Cycle 2	96.1	56.6	10.7	3.1
Cycle 3	31.6	61.2	21.7	23.5
Cycle 4	20.0	34.5	12.9	3.3
Av. of 4 cycles	38.0	46.7	12.5	13.2

Percentages above Differences are of Amounts *Saved* under *Rest* Condition: Relative Inhibition

Length of Lists	4 numbers	6 numbers	8 numbers	10 numbers
Cycle 1	61.6	54.2	8.0	27.2
Cycle 2	169.8	87.2	14.5	4.7
Cycle 3	86.3	70.6	31.9	28.8
Cycle 4	133.3	46.1	17.2	4.4
Av. of 4 cycles	112.7	64.5	17.5	16.3

recall or *saving* under the *rest* condition *minus* the *recall* or *saving* under the *work* condition. The relative amount of inhibition, on the other hand, is the percentage which the above difference is of the *recall* or *saving* under the *rest* condition. Tables VI and VII show the absolute and relative amounts of retroactive inhibition according to *recall* and *saving*.

If we consider the lists containing 6, 8, and 10 numbers, all of our methods of measuring retroactive inhibition indicate with some consistency a decreasing susceptibility to this form of inhibition with increasing length. It is only according to relative inhibition in terms of *saving*, however, that the inverse relationship between length and susceptibility to retroactive inhibition includes the 4-number lists. All other evidence points fairly definitely toward a lower limit to this relationship.

In our experiment with nonsense syllables there was also a tendency toward decreasing susceptibility to retroactive inhibition with increasing length, and there was some evidence for a lower limit to this tendency. The facts are interesting in themselves; but their significance is possibly greater if they are considered in connection with forgetting as it occurs under more normal conditions. There is a perfectly respectable theory of retroactive inhibition which says that this interference effect, while quantitatively more pronounced, is essentially the same sort of interference lying at the bottom of most of our forgetting. If that is so, we might have some reason to expect a relationship between retroactive inhibition and length similar to that between normal forgetting and length. Now we *have* found the most prominent tendency in the one case to be decreasing inhibition with increasing length and in the other decreasing forgetting with increasing length. But for the lower limit to the

more general relationship between length and retroactive inhibition we have found no counterpart in normal forgetting. Why this is so we shall not at this time even hazard a guess.

VI. Summary of Results

1. The absolute curves for memorizing numbers are like those for memorizing nonsense syllables in regard to the following particulars. (*a*) The curves for lists of different lengths all show regular, negative acceleration. (*b*) The shorter the list, the more rapidly does learning proceed in its early stages.

2. A consideration of the percentages of each list learned in selected percentages of the total time required to learn that list reveals practically coincident curves for lists containing 6, 8, and 10 numbers, and another set of practically coincident curves for lists containing 9, 12, 15, and 18 nonsense syllables. Lists containing 4 numbers and lists containing 6 syllables fail to conform to this law.

3. The amount of time required for memorizing increases at an increasing rate as the number lists become longer. The same law holds for nonsense syllables.

4. There is a general, but not very consistent tendency for the rate of forgetting to vary inversely as the length of the number lists. A similar tendency was found for nonsense syllables.

5. In general there is a decreasing susceptibility to retroactive inhibition as the number-lists increase in length. There is good evidence, however, of a lower limit to this law. These facts are in accord with those found for nonsense syllables.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY

Communicated by EDMUND C. SANFORD

XXVII. A STUDY OF 100 CONSECUTIVELY RECORDED DREAMS[1]

By ATTILIO RIZZOLO

I. Dream Facts

These 100 dreams were collected between Nov. 21st, 1921 and Feb. 27th, 1922 by the simple expedient of keeping writing material at the bedside, making rough notes of the dream at once upon waking, and writing up these notes next morning as fully as memory would permit, at which time also explanatory data when available were jotted down. While material of this sort is, of course, inadequate for a thorough statistical study of dreams, both because of the limited number recorded and because the dreams are those of a single dreamer, it is, nevertheless, quite sufficient for a test of the adequacy and universality of current dream theories and for a rough indication of the variety and complexity of the phenomena to be accounted for.

The material gathered has been studied with reference to *excitation* (*i e.*, the extent to which the dreams or features in them could be traced to sensory stimuli received during sleep), to *motivation* (*i.e.*, the extent to which the dreams were colored by desires or other emotions), with reference to their *effect* in producing noticeable movements, to the involvement of *'higher' mental processes*, to their *logical or absurd character*, to the *chronology of the influences* determining them, and to such general conditions as *fatigue, wakefulness*, etc. The main categories together with typical examples follow.

(1) *Dreams excited in whole or in part by sensory stimuli.* Of this familiar type the collection offers a few examples. Dream 17 (first part) is typical:

> The dreamer dreams that his small sister is playing an old melody upon a piano which his people have recently purchased. At the conclusion of the melody the dream changes.

[1]This paper is an abstract of a thesis submitted in partial fulfillment of the requirements for the degree of Master of Arts at Clark University, 1922. The thesis in full (pp. 108, with bibliography of titles in English) is on file in the Library of Clark University and the complete protocol of the dreams is in the archives of the Clark Psychological Laboratory.

Remarks. The dreamer was awakened about 3 a. m. by the playing of a piano in an adjoining room. Although the disturbance continued, he fell asleep after 15 or 20 min. and dreamed the above dream.[2]

In four instances such a causation seems reasonably clear. It may have been operative in three others, but a positive statement is impossible.

(2) *Dreams involving a carrying over of experiences and emotions from the waking state to the dream.* Of these dreams two sorts may be distinguished: one in which the dream repeats with some exactness an experience of the preceding day or hour—thus one in which 'recency' may perhaps be a factor—and another in which the dream falls in with, or gives expression to, a more or less persistent mood or emotional attitude—thus one that would follow the laws of 'frequency' and 'vividness'.

Dreams of the first sort are fairly numerous among the 100 recorded. As an example dream 64—which is also interesting as seeming to be nearly free of the element of desire—will serve.

The dreamer dreams that his room-mates are discussing the teaching methods of a certain professor. They describe, criticise and condemn the methods. The dreamer takes no part in the discussion and passes no judgment upon either the methods or the criticisms made by his room-mates.

Remarks. During the preceding day certain remarks were made by the dreamer's room-mates in reference to the teaching methods of the professor in question. The room-mates maintained that the methods were ineffectual. The dreamer listened to the remarks but did not give his opinion, remaining silent throughout the entire discussion.

Dreams in which an emotional attitude has been efficient in determining the dream content were also very common. In most instances the emotion can, for our purposes, be regarded as some sort of a desire. The term 'desire' belongs, of course, to popular psychology and so lacks precise definition. It stands nevertheless for a well recognized emotional condition and one to which great importance has been assigned in some recent dream theories. The writer will use the term here in a broad way for all mental processes characterized by unrest, discomfort or dissatisfaction,—for all those processes which would tend, if they resulted in action, toward the removal of the disturbing features of the situation. It is evident that under such a definition the only mental states that would *not* be states of desire would be those of rest, comfort and satisfaction,—such processes as would lead, if they should result in action at all, to action favoring the preservation of the *status quo*. He will re-

[2]The wording of the dream records as here presented, as well as that of the text in general, has been revised by Dr. Sanford, but the revisions in the former have been carefully checked over by the writer and have been accepted only when found to convey the full intent of the original record without distortion. The records filed in the Clark Laboratory remain, of course, as originally worded.

gard a dream as motivated by desire when the dream, if it were real, would be in some degree a satisfaction of the desire in question: a dreamer, for example, goes to bed hungry and dreams of a feast. Within the general class of desire-dreams it will be convenient for purposes of presentation to recognize several sub-classes depending, first, upon the extent to which the satisfying of the desire is impeded in waking life; secondly, upon its intensity; and thirdly, upon its object. The next five sections will therefore deal with dreams motivated by: (*a*) simple desires; (*b*) repressed desires; (*c*) suppressed desires: (*d*) impatient desires; and (*e*) desires of particular sorts.[3]

(3) (*a*) *Dreams motivated by simple desires.* The simple desire is always of relatively low intensity and lacks the element of anxiety. It is also usually of short duration. As an example, dream 40.

The dreamer dreams that the piano in his home has been sent to a piano factory for repairs. In this dream he sees the workman putting in new keys, tuning the piano, polishing it, etc.

Remarks. On returning home from the University the dreamer notices that the piano needs repairing and speaks to his sister of wishing to send it to the factory. His sister agrees with him as to the need and both decide to wait for an opportune time to send it. The desire does not rise to any considerable degree of intensity.

(3) (*b*) *Dreams motivated by repressed desires.* Dreams of this sort are extremely numerous in the present collection and a large number could be cited. Instead, however, of offering further miscellaneous examples the writer ventures to give the following seven dreams from a longer sequence, which are interesting in having for motivation a single persistent repressed desire which was at first of high intensity, but later ebbed, and was finally replaced at least temporarily by an emotion of contrary character. He apologizes for their intimate nature. It is sufficient to say, by way of general introduction, that a few weeks before taking up the study of dreams the dreamer had been jilted by his sweetheart. The dreams are at first motivated by an intense desire to regain her favor. In the full thesis,

[3]By a *repressed* desire the writer means a desire the prompt fulfillment of which in waking life is recognized as highly improbable. It differs, on the one hand, from a *simple* desire (*i.e.*, from a natural desire whose fulfilment is confidently looked for in the natural course of events) and, on the other hand, from a *suppressed* or hopeless desire,—a desire the fulfilment of which is regarded as so completely impossible that it no longer tends to modify action. These three classes pass into one another, of course, by imperceptible gradations; a simple desire becomes repressed where its satisfaction is unexpectedly postponed, and may even become suppressed when circumstances are unfavorable (in which case it usually disappears entirely); while a repressed desire may waver between the simple condition and suppression, as hope rises and falls.

the sequence consists of 19 dreams: 8 from the first quarter of the records; 4, 3 and 4 from the succeeding quarters.

Dream 2. The dreamer dreams that he is attempting to regain the love of his former sweetheart. She responds by telling him never to speak to her again.

Remarks. The dreamer was in a state of high nervous tension. Since the moment she left him he had endeavored to discover the motive for her act.

Dream 10. The dreamer dreams that he meets his former sweetheart in a playground where both had worked together. He reproaches her for her conduct toward him, whereupon she begins to weep. Seeing her in tears the dreamer apologizes for his reproaches. His apology is accepted and he asks her to meet him at once in the auditorium. She accepts the invitation and meets him there. Feeling secure from suspicious persons they embrace each other.

Dream 11. The dreamer dreams of a certain doctor and his son. The son is driving a Fiat car through the street on which the dreamer's former sweetheart lives. The sight grieves him, for he fears that she will fall in love with the doctor's son.

Remarks. In the preceding waking state the dreamer was reminded of the doctor and his son, and immediately afterward felt again the desire to regain his lost sweetheart.

Dream 19. The dreamer dreams that in order to regain the love of his former sweetheart he courts another lady, hoping to make her repent her former action and return to him. As he is in the act of kissing this second lady his sweetheart interrupts him. She is grieved and begins to cry. The dreamer seeing the success of his plan embraces and kisses her. She reciprocates.

Remarks. In the evening preceding the dream the dreamer had thought of trying a stratagem like that executed in the dream.

Dream 48 (first part). The dreamer dreams that he encounters his former sweetheart. Ignoring him, she approaches another young man whom the dreamer recognizes as Mr. Z. whom he had met during the preceding waking state. Her conduct with Mr. Z. is rather free and she finally embraces him. This is a great shock to the dreamer. He is depressed and concludes that he will never be able to regain her love.

Remarks. During the day preceding, the dreamer was told by his sister that his former sweetheart would never return to him. Feeling depressed the dreamer set out for a walk through a certain park. As he was walking Mr. Z. approached him. In the conversation which followed he discovered that his former sweetheart had corresponded with this young man. Putting this with his sister's statement he begins to fear that his former sweetheart will indeed never return to him. He is still in this mood when he goes to bed.

Dream 75. The dreamer dreams that he is attending one of Dr. B's lectures. After the lecture as he walks toward the University building, he meets his former sweetheart whose face is blackened with charcoal. She endeavors to speak to him, but he ignores her and continues his walk toward the building. On entering he discovers that his people are living in the gymnasium and that they are making preparations for a gorgeous feast. Different kinds of meat and pastry are strewn over the floor. After 10 or 15 min. in the gymnasium the dream changes.

The dreamer dreams that he is attending a lecture on the nature of man by Dr. X. His sweetheart is also at the lecture. She tells the dreamer that she is there to see him reduced to insignificance by Dr. X. During the first

part of the lecture, the dreamer makes no comment on what the lecturer says, but during the second part he challenges every statement. He tells the lecturer that he knows nothing of human nature, that he is nothing but a statistical sociologist, and that since he has never taken courses in anatomy, physiology, neurology, embryology, physical anthropology, heredity, biology and abnormal psychology, he should not attempt to explain human behavior. After having reprimanded the professor for his rash statements, the dreamer notices that his former sweetheart is laughing at him. Angered at the thought of being an object of ridicule in the mind of his former sweetheart the dreamer leaves the room. As he is descending a flight of nearby stairs he discovers that he is wearing his pajamas and bath robe. The shock produced by this discovery wakes him up.

Remarks. In the evening before this dream the dreamer had had a digestive upset and was in a condition bordering on neurasthenia from overstudy.

Dream 100. The dreamer dreams that his former sweetheart is attempting to regain his love. She approaches him and attempts to embrace him. As she is about to do so, he pushes her aside and expresses his desire never to see her again.

Remarks. During the day preceding this dream the dreamer experienced a strong aversion to his former sweetheart. The thought of having been unjustly jilted was the basis of the aversion.

(3) (*c*) *Dreams motivated by suppressed desires.* As an example of a dream embodying a suppressed desire the following is cited:

Dream 4. The dreamer dreams that he has received a book which was promised to him by one of his friends but which was never received. He received the book indifferently in spite of the fact that before the suppression of the desire for it he had impatiently awaited its arrival.

Remarks. After having looked for the arrival of the book for three or four weeks, the dreamer received a letter from his friend expressing his inability to lend or to give him the book. The dreamer became angry and determined to forget both the friend and the book. As time passed, though incapable of forgetting his friend, he did actually forget the book. The suppressed and forgotten desire obtained satisfaction in the dream.

Desires of a high degree of intensity are rarely wholly suppressed—they are as enduring as hope—and so dreams motivated by suppressed desires are rare in the present dream collection.

(3) (*d*) *Dreams motivated by intense or impatient desires.* As an example of this class the writer offers dream 15.

The dreamer dreams that he has received from his mother jars containing a special kind of chicken soup thickened with a special kind of spaghetti. The sight of the jars delights him. He opens one of them and partakes of its contents.

Remarks. The dreamer, suffering from a chronic intestinal trouble, had had a special diet prescribed for him by his physician, which included the soup mentioned. The dreamer gave the prescription to his mother who promised to send him the soup after his return to Clark University. After his return the dreamer became intensely anxious to receive the soup. From early in the morning till late in the afternoon he was on the look-out for a parcel containing it, and as days passed without its arrival the desire gained still more intensity. Finally it induced the dream recorded.

(3) (*e*) *Dreams motivated by sex desires.* This sort of desire stands out in some measure from the rest and to it, in the dream theories of Freud especially, considerable importance has been attached. Here it will be convenient, after the manner of the biological studies of sex differences, to separate the primary sex desire from the various secondary sex desires, the former expressing the reproductive instinct in its immediate and undeveloped form, the latter in its remote transformations and sublimations.

In the present collection of 100 dreams 6 are dominated by sex emotion of the primary sort. In 5 of the 6 the desire for primary sexual gratification had been present during the day preceding the dream. In a single case the preceding sex desire was of the secondary sort. In 3 of the dreams the full sexual act was dreamed as accomplished; in 3 it was dreamed as having been prevented, once by the entrance of a third person and twice through a moral struggle.

Of dreams motivated by secondary sex desires, such as romantic love, or desire for the society or friendship of the other sex, the collection shows a number of instances. Examples from the largest single group of these, those having reference to the lost sweetheart, have been already given. The following, dream 9, may serve as a further instance.

The dreamer dreams of a teacher who was fond of him when he was a boy of eleven. He pays her a visit at her school. She is delighted to see him and invites him to stay. He accepts the invitation and stays till school closes. After the children are gone he takes a short walk with her. While they walk a conversation begins in which each relates interesting experiences to the other. After the walk the dreamer escorts the teacher to her home.

Remarks. As the dreamer lay awake very early one morning he thought of a certain teacher. This thought brought to mind the teacher who appeared in the dream and the dreamer fell asleep with the thought of the latter in his mind.

(4) *Dreams resulting in muscular movement.* The number of dreams which were noted as leading to overt muscular movement was very small, probably through failure in observation. Such movement was present, of course, in the primary sex dreams and may be inferred with a good deal of certainty in dreams which ended in a "shock" of any sort; but it is recorded in only one or two instances.

(5) *Dreams involving the 'higher' centers.* Several dreams present the dreamer as taking part as speaker and listener in discussions of highly technical subjects, and maintaining his positions with pertinent arguments. The following are examples from a considerable number which might be given.

Dream 42. The dreamer dreams that he is discussing a phase of abnormal psychology with a group of friends. He is asked whether the corti-

cal cells which have remained undeveloped in the brain can be stimulated to normal development. He answers that there is a possibility, but that the stimulation of the undeveloped cells depends upon the stimulation of the normally developed cells, and this in turn on many conditions. He mentions two: first, whether the arrested development is due to heredity; and, second, whether it is due to internal environmental factors (*i.e.*, congenital factors) or to external environmental conditions (*i.e.*, factors which influenced the individual after birth). He asserts that if the factors are hereditary, the possibility of stimulating the arrested cells to normal development is small, in most cases *nil*; but that if the factors are environmental, the possibility is greater.

Remarks. During the evening preceding the dream the dreamer had been reading Donaldson's "Growth of the Brain", and while reading the account of the development of the cells had formulated the above hypothesis as regards the undeveloped cells. On going to bed the hypothesis still possessed his mind and he earnestly desired to know its validity.

Dream 43. The dreamer dreams that he is pondering over the hypothesis presented in the preceding dream. He criticizes it in every respect. At times he finds a possible fallacy in it; at other times he is inclined to believe in it. After weighing and considering its merits and defects he finally gravitates toward acceptance.

Remarks. During the day preceding the dream the dreamer continued his reading in Donaldson, was reminded of the hypothesis which he had formulated, and began to question and to doubt it. He remained doubtful through the rest of the day and had the doubt still with him when he fell asleep.

Dreams of this character, involving as they do such activities as comparison, criticism and the marshalling of arguments, are hardly comprehensible without the functioning of the 'higher' cortical centers. The quality of the functioning is, however, another matter. If there is thinking in dreams, as our collection seems certainly to show, it is, at least in the case of the present dreamer, something considerably short of thinking at its best.

(6) *Dreams showing confusion and absurdity.* The following may serve as an example:

Dream 56. The dreamer dreams that he is going to Detroit. He hires a small boat for the purpose of making the trip, and rows through canals, rivers and lakes until he comes to Boston. There he decides to make the rest of the journey by train. While waiting for his train he chances to cast his eyes toward the harbor and notices a peculiar thing. The gasoline tanks of the garages of Boston and its vicinity are located in the harbor a few hundred yards from shore. They look like floating cylindrical pontoons. At first he is puzzled as to what they are, but is told later by a passer-by that they are gasoline tanks. The train comes in on time and the dreamer leaves Boston. Throughout the entire dream the dreamer is conscious of his purpose to go to Detroit to see a friend who lives there.

Remarks. During the day preceding the dream the dreamer had had a strong desire to see his friend in Detroit. He had promised the friend to pay him a visit, but conditions had made the trip impossible and he realized this impossibility. During the same day he had inquired as to the various ways of going to Detroit. Some of his friends advised him to go by train, while others advised him to go from New York to Boston by boat and from Boston to Detroit by train. Desiring to make the trip as cheaply as possible he was inclined to the latter route.

(7) *Chronology of experiences upon which the dreams were based.*

Dreams based on immediate sensory experience.....1[4]
Dreams based on experiences occurring for the first time during the waking period immediately preceding the dream.....46

Dreams based on experiences occurring more than one day, or waking period, before the dream:

Interval of	2 days	2
"	" 3 "	2
"	" 2 weeks	2
"	" 3 "	4
"	" 3 to 4 weeks	1
"	" 1 month	10
"	" 2 months	2
"	" 3½ "	1
"	" 5½ "	1
"	" 7 "	2
"	" 3 years	1

Dreams based on experiences of childhood 3

Dreams based on experiences occurring for the first time during the preceding waking period and on experiences occurring within an interval of 1 year.....5

Dreams based on experiences occurring for the first time during the preceding waking period and on experiences of childhood.....1

Dreams based on experiences occurring within an interval of one month and on experiences of childhood.....2

Dreams based on experiences which cannot be accounted for in sequence of time.....5

II. Dream Theories

The first and perhaps the strongest impression produced by a large collection of dreams is the variety and complexity of the phenomena to be explained by any universal dream theory. The dream experiences are as varied as the experiences of waking life—more varied, indeed, since they include the absurd and the impossible. On this aspect of the matter the writer finds himself in complete accord with Prince, who expresses himself thus in the introduction to Mrs. Arnold-Foster's "Studies in Dreams":[5]

"The fact is, Mrs. Arnold-Foster hits the nail on the head when she says 'there are dreams and dreams, and we must get rid of the assumption that they all resemble each other.' This assumption is a very common one: in particular it is often assumed that a dream implies incongruity or incoherence, or the grotesque, or logical anarchy. Dreams as the author, Mrs. Arnold-Foster, stresses, may not only exhibit orderly imagination, reasoning, and memory and other qualities of the mind, but this imagination, reasoning and memory may be highly constructive, ingeniously inventive and producing imaginings or romances comparable in structure and sequence of ideas to stories of fiction and of real life evolved by the waking mind....

[4]Although several dreams of this sort were classified under this head for Section (1) above, all except one contained so many other elements that they have been placed in other groups in this chronology.

[5]Arnold-Foster, *Studies in Dreams*, 1921, 10 f.

"What is still needed, as the author has pointed out, is systematic and accurate recording of....dreams....and the correlation of the phenomena with identical phenomena occurring in certain other states of the waking mental life. If this were done, we should be surprised to find what a great variety of forms and structure dreams have, how greatly they differ in type, and in the mental processes involved, We thus should have the material from which we could safely construct theories of mechanisms that would satisfy the different types. After the collection of this varied material we could then begin, with greater safety, to analyse and interpret. In some we should find symbolism, in others none; in some repressed wishes, in others unrepressed wishes, or fears, doubts and scruples; in some sex urgings, in others the urgings of one or more of the other various innate instincts, which are the prime movers of human behavior; in some the solution of problems which have baffled our waking consciousness, in others the mere illogical fantasies of a weak, dissociated extract of our mental selves; in some the reproduction of memories, and living over again in the realistic form previous actual experiences; in others imaginary episodes of apparent super-knowledge constructed out of previous information; in some incongruous, grotesque phantasmagoria in cinema-like scenes, in others romances or well constructed fantasies requiring for their invention a large system of thought and an intelligence and imagination comparable to the waking self-consciousness. And in some we should find that the dream, as in waking life, is only the manifested expression of deeper-lying subconscious processes; and in some, probably in most, that it is just what it appears to be—nothing more."

The most prominent of current dream theories is that of Freud. For him the dream is always a wish-fulfillment, sometimes simple and direct, as in the dreams of children, but more often in the case of adults (since the wishes that find dream expression for adults are often so crude and primitively sexual as to clash with deep-seated moral or other standards) appearing in transmuted and symbolic forms. Adult dreams owe much of this peculiar character also to the fact that the wishes underlying them run back to early childhood.[6] That many dreams are motivated by a wish or desire of some sort our collection abundantly demonstrates, and in so far supports the Freudian view; but it also shows a considerable group in which no certain element of desire can be traced—dreams which appear to be merely the re-living of casual waking experiences. Freud also recognizes the occurrence of such 'sober dreams,' but holds them

[6]Freud's theory is thus briefly stated by Woodworth (*Psychology*, 507): "All dreams are wish fulfillments, even those that seem mere fantastic play of imagination, since no mental activity can accrue except to gratify some wish. All dreams are fulfillments of suppressed wishes and these are either sex or spite wishes, the spite wishes growing out of the interference of other people with our sex wishes."

H. Ellis (*The World of Dreams*, 165) summarizes it thus: "Freud goes far beyond the fundamental....proposition that the dream-imagery is largely symbolic. He holds that behind the symbolism of dreams there lies ultimately a wish; he believes, moreover, that this wish tends to be really of more or less sexual character, and, further, that it is tinged by elements that go back to the dreamer's infantile days....Thus Maeder puts it,

(*Footnote 6 continued on page 253*)

to be a small minority.[7] Dreams which are simple and undistorted wish-fulfillments he regards as characteristically the dreams of children. But our collection seems to show him to be in error here, for three-quarters of our dreams are of this undistorted sort. Much the same must be said with reference to the presence of factors derived from the experiences of childhood. In the undistorted dreams such factors are extremely rare. Whether they are more frequent even in the distorted dreams is certainly an open question for anyone who is not already a convinced adherent of Freud.

In the full thesis a summary of a considerable number of dream theories from the current literature of the subject follows at this point. Most or all of them set forth views in some measure at variance with those of Freud, and nearly all would find support in some measure in the 100 dreams of this collection.

An appendix deals with the relation of the frequency and character of the dreams to the length of the preceding period of sleep and to the physical condition of the dreamer. It is obvious that in order to record a dream the dreamer must rouse himself sufficiently to take pencil and paper and write; and dreams slip from memory with extreme facility. It is practically certain therefore that every recorded dream is a dream which occurred not long before waking. If, further, the dreamer wakes spontaneously (as was the case for all the dreams of our collection except one), it is only dreams which occur after a period of sleep sufficient to allow the dreamer to wake spontaneously that get into the record. Whether the earlier and deeper stages of sleep are dreamless or are attended by dreams that are forgotten our records furnish no basis for conjecture. It is interesting, however, to consider the length of the sleep period preceding the recorded dreams and the relation of the latter to

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summarising Freud's views, 'behind the insignificant events of the day utilized in the dream there is always an important idea or event hidden. We only dream of things that are worth while'. . . ."

Freud himself (*General Introduction to Psychoanalysis*, 1920, 161) denies that he regards all dreams as sexual. He says: "You must surely have heard that in psychoanalysis it is always maintained that all dreams have a sexual meaning. Now you yourselves are in a position to form a judgment as to the incorrectness of this reproach. You have become acquainted with the wish-fulfillment dreams, which deal with the satisfying of the plainest needs, of hunger, of thirst, of longing for freedom, the dreams of convenience and of impatience and likewise the purely covetous and egoistic dreams. But that the markedly distorted dreams preponderantly . . . .though again not exclusively—give expression to sex wishes, is a fact you may certainly keep in mind as one of the results of psychoanalytical research."

[7] *Op. cit*, 75.

the physical condition of the dreamer, and this our records permit. A study of the data seems to justify the following conclusions:

(1) When only one dream occurs on a given date and the physical condition of the dreamer is normal the average amount of sleep preceding the dream is about 4 hrs.

(2) When only one dream occurs on a given date and the dreamer is suffering from either fatigue, emotional excitement, physical pain, insomnia, physical or nervous exhaustion—or from a combination of these disturbances—the average amount of sleep preceding the dream is nearly 5½ hrs.

(3) When the dreamer went to bed in a state of considerable fatigue he was more apt to dream than when his condition was normal. When normal he usually recorded but one dream a night; when fatigued he noted two dreams, and on some dates three.

Comparing the normal and fatigued conditions in the light of these conclusions, we seem justified in saying that when the dreamer was fatigued he slept for a longer period before reaching a stage at which he could record a dream, but that, having recorded one, he was more apt to record one or two more. It is perhaps a reasonable conjecture that this difference points to a difference in the shape of the sleep curve (depth of sleep) in the two conditions.

A study of the character of the dreams—logical or absurd—in connection with the physical state of the dreamer on going to sleep seems to justify the following statement. Out of 105 dreams[8], 55 occurred when the dreamer was in a normal condition and 50 when he was suffering from fatigue or some other untoward condition. The proportion of illogical, jumbled and absurd dreams in each class is as follows:

Normal condition: 55 dreams, of which 4 were absurd and 2 partly so.
Abnormal condition: 50 dreams, of which 15 were absurd and 3 partly so.

The abnormal conditions may be further specified as follows:
Moderate fatigue: 23 dreams, of which 3 were absurd and 1 partly so.
Extreme fatigue: 23 dreams, of which 12 were absurd and 2 partly so.
Emotional excitement or physical pain: 4 dreams, all quite logical in character.

If we take all cases together, it is clear that nearly all of the present writer's dreams were logical if his general state was normal, and that the proportion of illogical, absurd or jumbled dreams increased rapidly with increasing fatigue. For normal conditions the ratio of logical to absurd dreams was about 9:1; in moderate fatigue about 6:1; while in extreme fatigue the ratio was reversed, the absurd dreams preponderating in about the proportion of 5:3.

[8]The number 105 includes 5 dreams not considered in the earlier sections of this paper because they were too incompletely remembered to be written out. The dreamer knew that he had dreamed something jumbled or absurd, but could recall no more.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF PITTSBURGH

Communicated by GILBERT J. RICH

I. FURTHER DATA FOR AN ASSOCIATIVE LIMEN

By J. L. ERNST, F. E. SMITH, L. R. MOESSNER, E. S. RUDISILL and M. J. ATWATER[1]

Williams[2] has shown that a modification of psychophysical procedure may be used in the measurement of the strength of association, and that the 'mneomometric function' and statistical limen furnish useful measures of associative strength. It is needless to repeat here his arguments in favor of such procedure. Williams, further, gave data derived from a short experiment by the method he proposed, and attempted so to state the unit of measurement as to fit the *phi-gamma* hypothesis to his curves. His work was admittedly tentative in its nature. The purpose of the present study is twofold: first, to gather additional data for the calculation of an associative limen, and, secondly, to try out a number of mathematical procedures in the statistical treatment of the data obtained. We have extended the previous experimental work by obtaining data from a considerably greater number of observations, and by obtaining 'mnemometric functions' for speed of presentation as the variable as well as for number of repetitions as the variable. We have then applied a number of mathematical procedures to the computation of a limen and tested the accuracy of fit of the curves so obtained to the observed data.

Four *O*s took part in the experiment, all of whom were graduate students in psychology with at least one year of laboratory training. E and S acted as *E*s for each other, as did M and R.[3] Every *E* made up his own series of nonsense syllables, so that as *O* he learned syllables with which he had no previous acquaintance. The 'mnemometric functions' for variable number of repetitions as the measure of associative strength were obtained from *O*s S and M, the functions for variable speeds or presentation as the measure from *O*s E and R.

The material consisted of series of 10 nonsense syllables, constructed to Gamble's modification of Müller and Schumann's rules.[4] The general procedure was that of the *Treffermethode*. The *O* learned a series of 10 syllables in trochaic rhythm at a given rate and for a given number of repetitions. He was not told in advance the number of repetitions or the speed of presentation (whichever was the variable), so that his attitude in learning was kept as constant as possible. Beginning 15 sec. after the last repetition, the *O* was presented with the first, third, fifth, seventh, and ninth syllables

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[1]Messrs. Ernst, Smith, Moessner and Rudisill performed the experimental work; Miss Atwater made the computations.

Our thanks are due to Dr. E. G. Boring of Harvard University for his advice during the prosecution of the work.

[2]H. D. Williams, On the Calculation of an Associative Limen, this JOURNAL, 29, 1918, 219ff.

[3]The experiments of E and S were performed in 1920–21; those of M and R during 1921–22.

[4]E. A. McC. Gamble, *Psychol. Rev. Monog.* 43, 1909, 18 ff.

of the series just learned in haphazard order, and was required to respond to each syllable with its associate. The associates were scored as right, half-right, or wrong.

The syllables used by E and S were typewritten on discs and presented stepwise in a Ranschburg memory-apparatus governed by a metronome. At the conclusion of every repetition, *E* turned the disc until he again came to the beginning of the series, and started the apparatus after such a time that each repetition was separated from the next by the time required to expose 4 syllables. The syllables of the test series were typewritten on separate slips of paper, which were laid before the *O* one after the other in making the test.

The syllables used by M and R were presented stepwise on a Spindler and Hoyer exposure apparatus. The series of typewritten syllables were presented on a 14-syllable drum, so that, with the drum running continuously, each repetition was separated from the succeeding by the time required to expose 4 syllables. The test series was placed on the same piece of paper, but each series (the learning series and the test series) was covered from view while the other was being presented. For all *O*s the successive members of the test series followed one another as rapidly as the *O* made his response or indicated that no associate was forthcoming.

The instructions to E and S were as follows. "After the signals 'ready' and 'now' there will be presented to you successively a series of nonsense syllables. This series will be on the revolving card of the machine and you will see one syllable at a time. You are to fixate the window and pronounce the syllables in trochaic rhythm as they appear. Give full and equal attention to every syllable. After a number of repetitions, the first members of the pairs will be presented. When the syllable is presented, pronounce as quickly as possible the syllable which followed it in the series, After you have pronounced it, write it down. If you do not know the syllable which followed, say 'Don't know,' and put a dash on your paper." For R and M, these instructions were modified to meet the type of apparatus used.

Every *O* learned from 100 to 200 series of syllables by way of practice before proceeding to regular series. These practice-series were used to determine the most advantageous speeds and numbers of repetitions for our purpose of computing limens, and these two factors were varied during the preliminary work.

In those experiments in which number of repetitions was the variable quantity, 5 numbers of repetitions were used, namely, 2, 4, 6, 8, and 10 repetitions. The rate of presentation was kept constant at .75 sec. per syllable for S and .8 sec. per syllable for M. S learned 40 series (or 200 pairs of associated syllables) at every number of repetitions, and M learned 50 series (or 250 pairs of associated syllables) at every number of repetitions.

In the experiments with speed of presentation as the variable, every series was learned for 5 repetitions. For E, the 5 rates of repetition used were .50, .75, 1.00, 1.25 and 1.50 sec. per syllable (120, 80, 60, 48 and 40 syllables per min., respectively). These same speeds were tried for R in the preliminary work. It was found, however, that his percentages of correct responses for the two slowest speeds, 1.25 and 1.50 sec. per syllable, were less than the percentages of correct responses for 1.00 sec. per syllable and higher speeds. Since this result is in conflict with previous experimental work, which has shown that strength of association varies inversely with speed of presentation,[5] we asked the *O* for an introspective account of his attitude in learning at various speeds. He reported that, while learning at the slowest speeds, the time between the pronunciation of one syllable and the appearance of the next syllable was so long that his attention

[5] R. M. Ogden, *Arch. f. d. ges. Psychol.*, 2, 1903, 93 ff.

wandered from the series to other ideas. He was not, then, learning the slow series with the same full attention with which he learned the rapid series. This report is in line with the accumulating evidence that attention is a condition of association.[6] We were concerned, however, in this study with strength of association as conditioned upon rate of presentation; and it was, therefore, necessary to keep all other conditions constant, especially the *O*s' attitude, attentional or otherwise.[7] From the report quoted above it appears that, for rapid rates of presentation to this *O*, attitude is a function of rate. What function it may be is a separate problem. We could not allow two variables, rate and attitude, to enter into our determinations, even though one may be a function of the other. We felt justified, therefore, in eliminating for this *O* the speeds at which his attitude was not constant. The 5 rates of presentation used in the experiments with R were: .50, .65, .80, .95, and 1.10 sec. per syllable (120, 93.1, 74.9, 63.1 and 54.5 syllables per min., respectively). E learned 40 series and R 50 series at each rate of repetition.

The primary results obtained are four 'mnemometric' functions, that is, percentages of right associates for every number of repetitions or for every speed of presentation, as the case may be. The number of repetitions (r) or speeds of presentation (s) and the resulting percentages of right associates (p) are as follows:

For S: r =	2	4	6	8	10	repetitions
p =	.2425	.4375	.5400	.7850	.8125	
For M: r =	2	4	6	8	10	repetitions
p =	.290	.484	.618	.756	.804	
For E: s =	.50	.75	1.00	1.25	1.50	sec./syl.
=	120	80	60	48	40	syl./min.
p =	.5300	.6950	.7775	.8050	.9100	
For R: s =	.50	.65	.80	.95	1.10	sec./syl.
=	1.20	93.1	74.9	63.1	54.5	syl./min.
p =	.338	.486	.554	.616	.784	

Inspection of the four functions shows no inversions of the first order, a fact which indicates relative constancy of attitude on the part of the *O*s, and, therefore, relative homogeneity of the data.[8] But, inasmuch as the functions contain inversions of the second order, it is evident that absolute homogeneity was not attained.[9]

The four functions given above represent all the facts which may be *experimentally* determined as a result of our procedure. They show the dependence of correct responses upon either the number of repetitions or the speed of presentation, as the case may be. It is desirable, however,

[6] A. Aall, *Zeit. f. Psychol.*, 66, 1913, 1 ff.; *Ber. ü. d. V. Kong. f. exper. Psychol.*, 1912, 237 ff. G. J. Rich, *Jour. of Educ., Psychol.*, 8, 1917, 239 f.

[7] S. S. George has shown the effect of attitude on psychophysical functions, this JOURNAL, 28, 1917, 1 ff. It is to be expected that attitude will similarly influence memorial functions.

[8] E. B. Titchener, *Experimental Psychology*, 2, 1, 1905, 94f. W. Brown and G. H. Thomson, *Essentials of Mental Measurement*, 1921, 81. Williams' data contain inversions of the first order for both *O*s. One *O* is definitely reported to have been unable to maintain a constant attitude; *op. cit.*, 221, 222.

[9] Brown and Thomson point out that homogeneity is rare in psychophysical data; *op. cit.*, 94.

to summarize the data into four limens or thresholds of associative effectiveness, that is, to state for every O the number of repetitions or the speed at which he will in one-half of the cases give a correct response. The determination of such a limen necessitates the assumption of some hypothesis toward the functions experimentally obtained. The point at which a correct response is as likely to be given as not to be given does not occur in the experimentally determined results, and it is necessary to assume that the required point lies upon some curve drawn to fit two or more of the experimentally determined points. The choice of the curve that shall be used is an arbitrary matter, but may be determined in part by the experience of other investigators in dealing with psychometric and 'mnemometric' functions. We have taken for trial 6 hypotheses with respect to the functions for repetitions and 4 hypotheses with respect to the functions for speed, have computed limens according to every one of these hypotheses, and have determined by two criteria the adequacy of every hypothesis to the experimental data.

Because of its frequent (and almost universal) use in psychophysics, we first chose the *phi-gamma* hypothesis[10] as a means of calculating the limens from the functions in which the number of repetitions was the variable. Next, we adopted the suggestion made by Williams that the frequency of correct response may be a compound function of the variable, such as the *phi-gamma* function of the logarithm of the repetitions or of the square root of the repetitions.[11] In the *phi-gamma* function as ordinarily used, and as used by us in the first computation of the limens, $\gamma = hr$. In our second and third computations of the thresholds, we have the additional assumption that $\gamma = h \log r$, and that $\gamma = h\sqrt{r}$. This meant that we used the logarithm or the square root of the number of repetitions in place of the number of repetitions itself, computed limens according to the standard procedure, and then found the antilogarithm or square of the resulting value, as the case might be.

Turning aside from the *phi-gamma* function, we assumed that the 'mnemometric' function was a portion of a straight line, determined by least squares (unweighted) the best fitting straight line (of the form $p = ar + b$) for the observed values, and found the value of r (repetitions) for which $p = 50\%$. This constituted our fourth calculation of the associative threshold. In making the fifth computation, we assumed that the function was of the logarithmic form ($p = a \log r + b$), and proceeded as in the preceding case. The sixth value of the limen for each O was similarly found by determining by the method of least squares (unweighted) the best fitting curve of the form $p = a\sqrt{r} + b$.[12]

Having determined the limens for the two Os, our next task was to test the adequacy of every hypothesis as a device for summarizing the experimental data. The adequacy is indicated in every case by the closeness of the curve to the observed values, that is, by the agreement of the values of p as calculated according to the given hypothesis with the observed values of p for the same number of repetitions. To obtain figures comparable with those of Williams, we have computed ϵ for every hypothesis ac-

[10] E. B. Titchener, *Experimental Psychology*, 2, 1, 92 ff; E. G. Boring, this JOURNAL, 28, 1917, 280 ff.

[11] We made no assumption with respect to a mnemometric unit, as did Williams, nor did we attempt to determine one.

[12] A linear, logarithmic or square-root function cannot logically be the mathematical representation of mnemometric data since it exceeds, possibly within the range of stimulation, the limits set by the experimental procedure ($p = .00$ and $p = 1.00$). A portion of such a function might, however, have fitted the observed values and have been, therefore, useful as a means of computation of limens.

cording to the formula which he gives.[13] Thomson has shown that this criterion is not the most adequate measure of goodness of fit. We have, therefore, calculated for every hypothesis the value of P according to Thomson's adaptation of Pearson's method of testing for goodness of fit.[14] The values of the limens (L) according to every hypothesis together with the measures of goodness of fit are given to the accompanying table.[15]

Hypothesis	*Observer S*			*Observer M*		
	L	ϵ	P	L	ϵ	P
Phi-gamma						
$\gamma = hr$	5.09	.048	.2208	4.57	.034	.3586
$\gamma = h \log r$	4.39	.063	.0499	4.52	.091	.0000
$\gamma = h\sqrt{r}$	4.74	.048	.2717	4.27	.015	.9454
Least Squares						
$p = ar + b$	5.15	.056	.0000	4.61	.046	.0000
$p = a \log r + b$	4.38	.062	.0000	3.96	.023	.0985
$p = a\sqrt{r} + b$	4.78	.049	.0000	4.29	.023	.0787

We may now turn to a consideration of the values obtained according to the various hypotheses. It is evident that the limen is dependent in large measure upon the hypothesis used. We are principally concerned, however, with the value of P. P is in every case the probability of obtaining in random sampling a fit as bad as or worse than that observed, if the hypothesis is a true representation of the facts. Thus, a P of .01 means that, if the assumed function were correct, in only one case out of a hundred should we obtain so bad a fit by mere chance, and therefore raises the presumption that the assumed function is not a correct representation of the relationship measured in the experiment. Or a P of .90 means that, if the assumed function were correct, in nine cases out of ten the fit would be no better than that obtained, and raises the presumption of adequacy of the hypothesis to the observed data. A high P means a hypothesis adequate to the facts which are to be summarized; a low P means an inadequate hypothesis.

For both *O*s, the most adequate hypothesis is a combination of *phi-gamma* and the square-root hypothesis, giving a P of .94 for M and of .27 for S. The first of these is unusually high for psychometric data of any sort. The next best fitting curve is that of the *phi-gamma* hypothesis taken alone, while the combination of *phi-gamma* and logarithmic hypotheses gives a probability of less than one in twenty of obtaining the observed values by chance.

For S, no one of the three remaining hypotheses proves to be of value in summarizing the data, for according to none is there as much as one chance in ten thousand of obtaining the observed data in a random sampling. For M, values of P as high as .09 are found, but even this figure represents less than one chance in ten of obtaining the observed values. We find, then, that mathematical functions of the *phi-gamma* type, especially when combined with a square-root function, are most adequate for the purpose of summarizing data of this sort and, therefore, for the computation of limens which represent the observed values; and that portions of simple non-'ogival' functions, linear, logarithmic or square-root, are inadequate for these purposes.

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[13]*Op. cit.*, 222.

[14]K. Pearson, *Tables for Statisticians and Biometricians*, 1914, xxxi. ff. G. H. Thomson, *Biometrika*, 12, 1919, 216ff. W. Brown and G. H. Thomson, *op. cit.*, 77ff.

[15]Linear interpolation gives limens of 5.12 for S and 4.24 for M; logarithmic interpolation limens of 5.12 for S and 4.20 for M; and square-root interpolation limens of 5.17 for S and 4.22 for M.

We may now consider the data obtained from the other two *Os*, in which rate of presentation was the variable factor. Two measures of this variable are possible: the time per unit of material read (sec. per syllable); and the amount of material read per unit time (syllables per min.). It is especially important to note that these two measures of the variable condition of association are not linearly related but are harmonic functions of each other.[16] It is to be expected, therefore, that the limens obtained as well as the adequacy of the hypotheses used will depend upon the measure of the stimulating conditions which is used in the computations. For convenience in comparison, all limens are expressed in sec. per syllable, results obtained by using syllables per min. as the measure being changed back to these units after the computations were completed.

As in dealing with the preceding data, we commenced with the *phi-gamma* hypothesis, using both measures of speed. Our third and fourth computations were performed by finding the best fitting straight lines by least squares (unweighted), for correct responses and sec. per syllable, and for correct responses and syllables per min., and finding on this line the speed at which p is 50%. The same measures of adequacy as already used (ϵ and P) were applied here, and are shown in the accompanying table together with the liminal values (L).[17]

Hypothesis	*Observer E*			*Observer R*		
	L	ϵ	P	L	ϵ	P
Phi-gamma						
Sec./syl.	.370	.033	.5701	.698	.043	.2885
Syl./min.	.487	.138	.2794	.666	.056	.0548
Least Squares						
Sec./syl.	−.249	.070	.0000	.714	.041	.0000
Syl./min.	.458	.034	.0006	.650	.063	.0001

The limens here again show dependence upon the hypothesis used, a dependence which is more marked in the case of E than it is in the case of R. This difference is due, probably, to the more adequate determination of the function as a result of the experimental work on R, for whom the speeds used were on both sides of the limen; while the speeds used with E all gave supraliminal learning.

When the *phi-gamma* hypothesis is used, it is more adequate to the observed data if the conditions of association are measured in terms of the time per unit read than if they are stated in amount read per unit time. We may state the same conclusion by saying that if syllables per min. are used as the variable, the combination of the *phi-gamma* hypothesis and the harmonic hypothesis is more adequate to the observed data than is the *phi-gamma* hypothesis alone. In the case of E, the better hypothesis gives a more than even chance of the observed data occurring in random sampling, while for R the probability is greater than one in four. The linear hypothesis, applied to either measure of speed, is inadequate to the facts, the probabilities of the observed data occurring under it being less than one in one thousand. In one case, the linear hypothesis leads only to the trivial solution of a negative liminal speed.

To summarize: we have obtained data for 'mnemometric' functions from 200 or 250 observations (grouped in series of 5 pairs of nonsense

[16] For (syl./min.) = 60 ÷ (sec./syl.); and (sec./syl.) = 60 ÷ (syl./min.)

[17] For R, linear interpolation in terms of sec./syl. gives a limen of .681, and in terms of syl./min. a limen of .685. For M, linear extrapolation from the two values nearest 50% gives a limen of .454 in terms of sec./syl., and of .468 in terms of syl./min.

syllables), and for two variable conditions of association; we have calculated limens from these data according to a number of hypotheses; we have found that, under our conditions of experimentation, a combination of the *phi-gamma* and square-root hypotheses is the most adequate of the means we have used to summarize the observations and thereby to calculate a limen for associative strength as conditioned upon number of repetitions; and we have found that the *phi-gamma* hypothesis applied to time per unit read occupies a similar position with respect to associative strength as conditioned upon speed of presentation.

A great deal of further investigation along the lines of procedure used in this study remains to be done. Our trial of various mathematical hypotheses was necessarily limited in its scope by the time at our disposal. It is possible that there are other hypotheses (such as the arctan, Pearsonian skew curves, etc.) which are more adequate to the problem than any we have used. These should be tried on data obtained from as large a number of observations as possible, especially as P becomes more sensitive as a criterion of fit as the number of observations increases. We have obtained results for only two variable conditions of association. The adaptation of psychophysical procedure here used may be applied to the measurement of other factors determining associative strength. A still further extension of the method would consist of the determination, for the same O, of the associative strength as conditioned at the same time upon two independently variable factors, as, for instance, number of repetitions and speed of presentation.

II. Constant *vs.* Accelerated Speed in Learning

By Anne E. Fineman and Selina Ruderman

Meumann[1] reports an experiment in learning nonsense syllables in which the O was allowed to control the speed at which the syllables were presented and was instructed to regulate it in such manner that he might learn most conveniently and might feel that he was making the most satisfactory progress. The results showed that the drum was rotated slowly at first and then more rapidly as the learning proceeded. Such results show only that an accelerated speed of learning was most comfortable to the learner, not that it produced the most efficient learning. Proof is lacking that the speed of presentation chosen by the O was the one at which he learned most efficiently, that is, that the O could choose introspectively the best speed for learning. It still remains an open question whether or not an accelerated speed of presentation actually results in more efficient learning than does a constant speed. We have attempted in a preliminary way to answer this question for a particular set of conditions.

The material to be learned consisted of nonsense syllables constructed according to Gamble's modification of Müller and Schumann's rules.[2] They were made up into series of 10 syllables and presented stepwise on a Spindler and Hoyer exposure apparatus. The 14 syllable drum was used, so that the successive repetitions of a series were separated from one another by the time required to expose 4 syllables. The procedure was that of the *Treffermethode*. The Os learned every series for 4 repetitions. Immediately after the completion of the fourth repetition, 5 of the syllables just learned (the first, third, fifth, seventh and ninth, in haphazard order) were presented, and the O was instructed to respond to every one with the syllable

[1] E. Meumann, *The Psychology of Learning*, 1913, 261f.

[2] E. A. McC. Gamble, *Psychol. Rev. Monog.* 43, 1909, 18ff.

that followed it in the series. Every test-syllable followed the preceding one as soon as the *O* gave the associate or indicated that no associate was forthcoming. Responses were scored as right or wrong.

Four constant and one variable (constantly accelerated) speeds were used. The constant speeds were: .50, .60, .75, .80 and 1.00 sec. per syllable (120, 100, 80 and 60 syllables per min., respectively). In the variable speed, the first repetition was at the rate of 1.00 sec./syl., the second at .75 sec./syl., the third at .60 sec./syl., and the fourth and last repetition at a speed of .50 sec./syl.[3] Each *O* learned 50 series (250 pairs of nonsense syllables at every speed). The order in which the various speeds occurred was determined by chance, but every speed was included in every set of 5 series in order to equalize practice.

Both *O*s (who also acted as *E*s for each other) had had a year of laboratory training which had included a similar experiment by the method of right associates, with the same apparatus. As both knew something of the problem, a procedure with knowledge was followed on the ground that a full knowledge of the experiment and results was more likely to lead to a constant attitude than was partial knowledge *plus* guesses, surmises and attempts to give the result which the *O* thought was desired. The *O*s, therefore, knew everything about the experiment except the syllables they were to learn.

Speed	*p*		Time for Learning	*p*/Time for Learning	
	F	R		F	R
(sec./syl.)			(sec.)		
1.00	.800	.540	40	.0200	.0135
.75	.776	.440	30	.0259	.0147
.60	.692	.412	24	.0288	.0171
.50	.568	.348	20	.0284	.0174
Variable	.704	.444	28.5	.0247	.0156

The accompanying table shows, in the second and third columns, the percentages of correct responses made by each *O* at every speed. For the 4 constant speeds, the percentages of right associates decrease with increase of speed. The lack of inversions of the first order indicates that fairly constant attitudes were maintained by the *O*s. The number of correct responses at the variable tempo are, for both *O*s, intermediate between the numbers for the slowest and the fastest tempos.

Our problem, however, is the relative efficiency of learning under the various presentations, that is, the ratio of amount learned to time expended.[4] As a basis for determining this ratio, we first calculated for every speed the total time of exposure of the 10 syllables during the 4 repetitions (excluding the time between repetitions) and considered this the time utilized for learning at that speed (fourth column). To obtain the efficiency of learning, we divided every percentage of correct responses by the time for learning. The resulting measures of efficiency of learning are shown in the fifth and sixth columns of the table. The relative efficiency of the constant speeds is not the same for the two *O*s. The discrepancy is understandable, however, when we compare the percentages of correct response. F learned a greater proportion of the syllables than did R, indicating that a speed which was slow for one was fast for the other. F reported, introspectively, that the slowest speed was so slow as to be annoying to her, while R did

[3]The speed, in terms of amount read per unit time, was *constantly accelerated*, as the four repetitions were made at 60, 80, 100 and 120 syl./min., respectively.

[4]This is Meumann's criterion of advantageous learning; *op. cit.*, 260.

not find this to be the case. R's maximum efficiency was not passed at a speed of .50 sec./syl., while F appears to have reached a maximum of efficiency in learning at .60 sec./syl.[5]

For both *Os*, however, the efficiency of learning at the constantly accelerated speed is intermediate between the highest and lowest efficiencies found for the constant speeds. The variable speed of presentation is more efficient than the slowest speed, but less efficient than the rapid speeds. Although the *Os* had, in the variable presentation, better opportunity to become familiar with the syllables during the earlier repetitions than in the case of the rapid presentation, the increased strength of association resulting therefrom does not compensate for the extra time consumed, so far as the ratio of amount learned to time expended is concerned.

The results of this experiment must, of course, be interpreted in the light of the experimental procedure. We used the method of right associates, not the method of complete mastery, and Ephrussi has shown that the effect of tempo on efficiency depends upon the method by which the strength of association is tested.[6] The values of the ratio of correct responses to time used for learning may or may not bear the same relations to one another as do the times required for complete learning. Moreover, our speeds were chosen within limited range. The fastest speed, .50 sec. per syllable, was not beyond the speed for maximum efficiency for one, and only slightly beyond it for the other *O*. The effect of a constantly accelerated speed of repetition which reached in the later repetitions a considerably higher rate than any we used is a matter for further investigation. The increasing familiarity of the material might possibly admit of the economical use of higher speeds.

We may then conclude that, when working by the method of right associates and with speeds not exceeding .50 sec. per syllable, a constantly accelerated speed of presentation will not produce in 4 repetitions as great an amount of material learned in proportion to time expended as will a constant rapid rate of presentation within the same limit. An adequate answer to the general question stated in our first paragraph requires further investigation with more rapid tempos, as well as by more than one method of testing retention.

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[5]R. M. Ogden has shown that a middle speed is more efficient than very rapid or very slow presentation. *Arch. f. d. ges. Psychol.*, 2, 1903, 93 ff.

[6]P. Ephrussi, *Zeit. f. Psychol.*, 37, 1905, 192; E. Meumann, *op. cit.*, 262.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF YALE UNIVERSITY

III. A Quantitative Experiment on the Purkinje Phenomenon

By Llewellyn T. Spencer

In the existing textbooks or outlines for courses in experimental psychology available to the writer no thorough experiment is given on the subject of the Purkinje phenomenon. The phenomenon consists in a loss of saturation when colors are viewed under decreasing illumination and also a shift of the point of maximal brightness of the spectrum from the yellow toward the green. Judd,[1] Seashore,[2] Hollingworth,[3] Langfeld and Allport,[4] and Poffenberger[5] make no mention of the phenomenon. Myers[6] and Foster[7] call the student's attention to the fact that a color mixture which matches gray in daylight fails to do so if the illumination is reduced. Sanford[8] and Titchener[9] direct the student to compare the brightness of red and blue in daylight and then note the change in brightness-relation upon entering the dark. The writer has found the following experiment practicable for groups of 14 or 15 students and clear results have been obtained with satisfactory uniformity.

The materials required are a set of colored papers and a two-candlepower lamp in a dark room, in series with which are a number of resistances which may be thrown into the circuit at will. The resistances used at the Yale Laboratory are arranged on two resistance boards, consisting of 100, 200, 300, and 400 ohms and 1000, 2000, 3000, and 4000 ohms. This makes it possible to obtain the series of 200, 400, 600, 800, 1000, 1200, and 1400 ohms used in the experiment. The set of colored papers is composed of five slips each, 1 by 1½ in., of red, orange, yellow, green blue, and violet, and also five each of white, gray, and black. These colors are chosen so as to approximate as nearly as possible the relative brightnesses of spectral colors.

The students are first asked to arrange the six chromatic papers in the order of their brightness, under daylight conditions, ranking the brightest as 6, the next as 5, and so on down to 1. All the slips are then thoroughly shuffled and the class enters the dark room. The two-candlepower lamp is suspended at some distance above the center of a table at which the students sit. The instructor throws in the resistances, as required, from a nearby table. Each part of the experiment is preceded by a 5 min. period of dark adaptation under the illumination to be used in the particluar observation. During the first of these periods, with a 200 ohm resistance in the circuit, the students are given the instructions for the experiment. At the instructor's signal they are to sort the colors by *hue* into piles, arranging the order of piles in the order of the colors of the spectrum to insure identification when the full lights are turned on. They are cautioned against (*a*) changing a judgment once it is made, (*b*) comparing a slip being considered

[1] Laboratory Manual of Psychology, 1907.
[2] Elementary Experiments in Psychology, 1909.
[3] Outlines for Experimental Psychology, 1914.
[4] Elementary Laboratory Course in Psychology, 1916.
[5] Experimental Psychology, Loose Leaf Manual.
[6] Textbook of Experimental Psychology, 1911, Part II., 32-33.
[7] Experiments in Psychology, 1923, 191.
[8] Course in Experimental Psychology, 1898, 142.
[9] Experimental Psychology: Instructor's Manual, Qualitative, 1918, 7.

with colors already sorted, (*c*) sorting by any other characteristic than hue, for example, by brightness, thickness, or texture, and (*d*) inferring the hue from the fact that five slips have already been sorted as of the color in question. They are requested to sort a slip into the colorless piles if they are unable to distinguish the hue with certainity. Guessing is forbidden. It is explained that the only function of the white, gray, and black is to provide places where unrecognizable colors may be sorted. It is found advisable to provide pieces of white cardboard upon which the sorting is done.

After the first sorting is completed the full lights are turned on and the sorting is scored. Record blanks ruled in 9 columns and headed with the 9 colors are required. The student lists under each color the number of slips correctly sorted into that pile, then the number and name of each color incorrectly sorted into that pile. The columns are totaled and the totals added. This should give the number of slips, *viz.*, 45. Then for each column the number of slips *correctly* sorted is divided by the number of slips *incorrectly* sorted *plus* 5, the number which should be sorted there. This percentage is thus a weighted score under which the student is penalized for not sorting the slips of the given color into that pile (by reducing the numerator) and also for sorting other colors into that pile (by increasing the denominator). Four red, one violet, and one blue in the red pile would score $\frac{4}{2+5} = 57\%$, while four orange slips in the orange pile would score 80%, and five orange would score 100%. The slips are then reshuffled, 200 ohms are added to the resistance, and 5 min. are again allowed for dark adaptation. This is done for the resistances of 200, 400, 600, 800, 1000, 1200, and 1400 ohms.

In treating the results the colorless slips are disregarded. The percentages of success for the 6 colors for each resistance are averaged, and the curve of accuracy is plotted against the degree of illumination. The percentage of accuracy for each color at the 1400 ohm level is plotted to obtain the curve of brightness at that level. This curve is compared with a similar curve plotted from the rankings of brightness made in daylight at the beginning of the experiment. The ranking figues, 6,5,4, etc., may be divided by 6 to render them comparable to the percentages of success in darkness when plotted on the same graph.

The following results were obtained by the writer with 6 subjects.

TABLE I

Red	*Orange*	*Yellow*	*Green*	*Blue*	*Violet*
Average rank for brightness under daylight conditions					
1.16	4	5.83	4.83	3.33	1.83
Average rank under daylight ÷6, then minus 7%, in percentages					
12	60	90	73	48	23
Average percentage correct with resistance of 1400 ohms					
13	47	52	90	26	16

In order to plot the values for daylight brightness on the same curve as the values for weak illumination, the ranks for the 6 colors must be converted to percentages. This may be done by dividing by 6, the highest possible rank. Since the values thus obtained give a curve slightly higher than the curve for 1400 ohms, they are best compared with the latter curve if some constant such as 7% is subtracted from each, so that the modes of both curves are of equal height. These are the results which are given in the second line of the Table.

The average percentage correct for the various resistances was for the 6 subjects:

200	400	600	800	1000	1200	1400
91.7	92.1	83.3	75.4	72.6	54	41

Plate I gives the data of Table I. The continuous line represents the brightness of the colors under daylight, and the broken line represents the average accuracy of the 6 subjects for each color with the 1400 ohm resistance. Plate II represents the average percentage of accuracy for all colors as the resistance in the circuit is increased.

It is evident that the experiment demonstrates the two significant facts of the Purkinje phenomenon, *viz.*, that the point of maximal brightness shifts from the yellow under daylight conditions to the green under weak illumination (*cf.* Plate I), and that the ability to distinguish the various hues decreases with the decrease in illumination (*cf.* Plate II).

The experiment takes about 90 min. of actual running time, so that, with a 15 min. period for instruction, etc., it may easily be done in a single 2 hr. laboratory period. There is sufficient intrinsic interest in the experiment for the student, in the writer's experience, to make its incorporation into the laboratory course very satisfactory as a quantitative experiment on the Purkinje phenomenon.

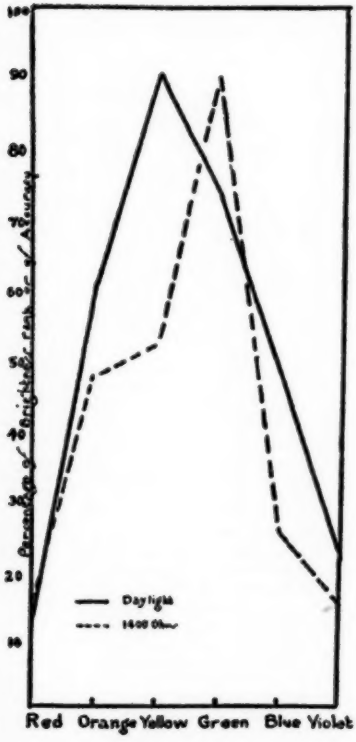

Plate I

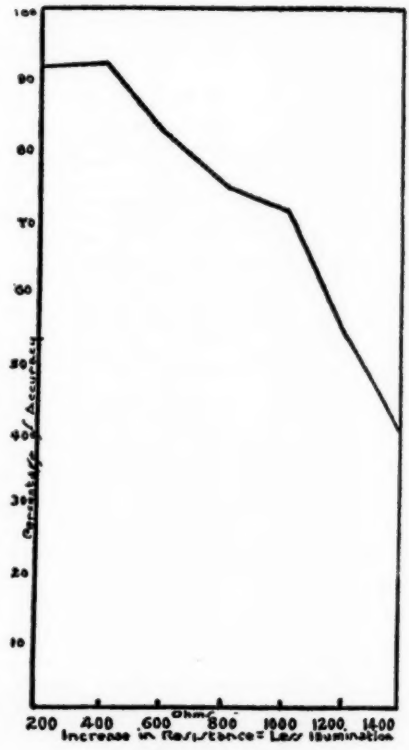

Plate II

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. TITCHENER

LXVII. POSITION *vs.* INTENSITY AS A DETERMINANT OF ATTENTION OF LEFT-HANDED OBSERVERS

By RUTH S. BURKE and KARL M. DALLENBACH

The study entitled "Position *vs.* Intensity as a Determinant of Clearness,"[1] recently published from this laboratory, showed conclusively that position was, within the limits of our experiment, a condition of attributive clearness and a determinant of attention; and that the left-hand position and the position above had an attentional advantage. As the *Os* who served in this study were right-handed, these results suggested a neurological explanation[2]; and is seemed worth while to ascertain the positional preference of left-handed *Os*.

Observers. The *Os* were Miss Clara C. Medlin (*M*) and Mr. Ernest R. Holmes (*H*), graduate students in the summer session of 1923.[3] H wrote with his left hand and was, on that basis alone, accepted as left-handed; but, as will later appear, he is not of the pure type. M is unquestionably of the pure type. She does everything but write with her left hand; sewing, buttoning, knitting, using knife and fork, wiping dishes, dealing cards, throwing, playing tennis, hammering, shoveling, etc., are all done with the left hand. Writing with her is a case of nurture overcoming nature; for until she was nine years old she also wrote with her left hand, but at that age she came under the instruction of a teacher who in accordance with the pedagogy of the time forced her to change.

We should have been glad to extend the experiment more widely; but, unfortunately for our purpose, these two *Os* were the only members of the summer classes in psychology, which numbered close to three hundred, who could qualify as left-handed.

Apparatus and Procedure. The same apparatus was used as in the earlier work, and the same procedure was followed: two circular areas, which varied only in position and in intensity of illumination, were compared; and the objective intensity at which the two were reported equally clear was determined for each *O* for every one of the four cardinal and four intermediate positions. The positions, as before, were: 1, horizontal, variable left, standard right of fixation; 2, sinistral oblique at 45°, variable left and below, standard right and above fixation; 3, vertical, variable below, standard above fixation; 4, dextral oblique at 45°, variable right and below, standard left and above fixation; 5, horizontal, variable right, standard left of fixation; 6, sinistral oblique at 45°, variable right and above, standard left and below fixation; 7, vertical, variable above, standard below fixation; 8, dextral oblique at 45°, variable left and above, standard right and below fixation. The length of exposure was, also as in the earlier study, approximately 0.1 sec.

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[1] K. M. Dallenbach, this JOURNAL, 34, 1923, 282ff.

[2] *Ibid.*, 286.

[3] Our thanks are due to Miss Medlin and Mr. Holmes for their co-operation and for the many hours that they spent in observation.

The following instructions, the same as those used in the previous experiment, were read to the *Os* at the beginning of every experimental hour: "At the signal 'Ready' fixate the spot of light before you; at 'Now' two areas of light will be shown; report which, if either, is the more clear. Give the position of the clearer as 'right', 'left', or 'above,' 'below', whichever the case may be; if they are equally clear, say 'same'; if you are uncertain, report 'doubtful.' Experiments reported 'doubtful' were, without *O*'s knowledge, immediately repeated; there were but few of these 'doubtful' reports.

After determining, in a series of preliminary experiments by the methods of limits, what the approximate intensive values of the variable necessary for equality of clearness would be for every one of the 8 positions, 5 stimulus-values were selected for each *O* for every position, and the main experiments were conducted, as in the earlier work, by the method of constant stimulus-differences. 40 observations were taken for *H* and 25 for *M* at every position for every value of stimulus. In accordance with the findings of the earlier experiment the positions as well as the stimuli were varied in haphazard order.[4] The *Os* never knew before the exposure what position the comparison areas would occupy.

Results. The results of this Study are shown in Table I, which gives for each *O* and for every position of the comparison areas the percent. of illumination necessary for the variable to be reported equally clear with the standard, the intensity of which was, as in the earlier study, 16%.

TABLE I

Showing for Each *O* and for Every Position of the Stimuli the Percent of Illumination of the Variable Necessary for the Report of Equality in Clearness

Position	Observer *H*		Observer *M*	
	% Illumination (L)	Precision (h)	% Illumination (L)	Precision (h)
1	12.5012	.1071	63.8136	.0396
2	9.5924	.1157	18.7536	.0379
3	18.7184	.0559	9.2192	.0740
4	40.2896	.0497	9.5236	.1154
5	24.8632	.0496	7.5964	.0900
6	17.5368	.0538	15.7928	.0602
7	17.0656	.0606	45.2392	.0529
8	9.9392	.1324	55.4568	.0482

These data show that position is effective for both *Os*; but also that it is effective in opposite directions. *M*, the undoubtedly left-handed *O*, is disposed toward the right. The areas to the right and below the point of fixation (positions 3, 4, 5 and 6) require an illumination of less intensity to equal the standard in clearness than do the areas above and to the left (positions 1, 2, 7 and 8). The most favorable position for *M* is 5, the position in the horizontal in which the variable is at the right of fixation; the most unfavorable is 1, the position in the horizontal in which the variable is at the left of fixation; the percent. of illumination required to equal the standard of 16% being for these positions 7.6% and 63.8% respectively. *H*, on the contrary, is disposed, as were the right-handed *O*'s, toward the left. The areas to the left and above fixation (positions 1, 2, 7 and 8) require less illumination to equal the standard in clearness than do those

[4] *Op. cit.*, 285.

below and to the right (positions 3, 4, 5 and 6). The most favorable position for *H* is 2, the position to the left and 45° below fixation; this result is in agreement with the results of the right-handed *O*s.

Time did not permit of an extended investigation of *H*'s type. We found, however, that he, unlike *M*, did many things right-handedly. He was for example, right-handed in batting, in swinging an ax or club, and in shoveling, and right-footed in kicking a football, and in jumping; he was also, as the results of this experiment show, right-handed in 'seeing.' He cannot, therefore, be regarded as a representative of the pure left-handed type, but belongs to some form (unfortunately, not further determined) of the intermediate type.[5]

Conclusions. We feel justified, on the basis of our experimental results with these two *O*s, in reaffirming the conclusion already drawn: that position is a condition of clearness and a determinant of attention; and in adding that the specific preferential position seems to be a matter of right- or left-handedness; the positions to the left and above have a distinct attentional advantage for right-handed *O*s, and contrariwise the positions to the right and below have an attentional advantage for left-handed *O*s.

LXVIII. The RL of Increased Chroma with Film Colors

By Diana Ginsberg and L. B. Hoisington

The problem of this Study grew out of the work of Elliott, West and Hoisington on the spatial limen for the four principal film colors.[1] With colored discs of equal tint and chroma their *O*s reported a pronounced increase in chroma as well as a luminous character when the stimulus color showed through a small opening in a screen of the same subjective brightness.[2] Our problem was to find, in terms of visual angle, the RL for this increase in chroma. In other words we proposed to increase the visual angle till the stimulus color shown through an opening should have the same degree of chroma as an identical stimulus standing free before the *O*. We hoped also to determine the nature of the psychometric function, if such a function should reveal itself, representing the changes in chroma when the stimuli are two film colors. Owing to unexpected complications, this part of the problem remains untouched.

The conditions were the same as, and the method similar to, those of the previous Study. We used the same apparatus with slight modifications, colored discs of the same composition, and the same sources of illumination with the fixed light in the same position as for the latter part of the previous work, except that we used 200 watt bulbs, and that the ends of the cones which confined the light had to be cut off to allow the illumination of a somewhat larger area on the screen.

Our problem demanded the use of two identical discs on separate motors; *O* was to judge the chroma of a film color in comparison with that of a color which entered into a total perception of surface. One disc stood just in front, and to the left of center, of the back member of the double grey screen, which was now covered over its entire surface with the no.6 grey. We shall refer to this as the perceptive stimulus: it presented to *O*, when the front screen was up, a colored surface like that of any ordinary rotating

[5] For types of dextrality see J. M. Rife, *Psych. Review*, 29, 1922, 474–480.

[1] This Journal 35, 1924, 125 ff.

[2] *Ibid.*, 131.

disc. The chroma of the color experience correlated with this stimulus served as standard of reference for the judgments. Just to the right of center in the back member of the double screen was a circular opening, 18 cm. in diam., which received cards, covered with the same grey, with circular openings at their centers of 31, 33, 35, 37 and 39 mm. diam. These dimensions resulted from a long series of preliminary, experiments; they gave the variable visual angles with the G stimulus. The second disc, which was 60 cm. behind the screen and which was the stimulus for the film color,[2] showed through the holes above mentioned. It was essential that these openings should be sharp cut and clean on the edges, so as not to offer contours or differences in tint. The front part of the double screen, which was covered over its entire surface with the grey paper, and which *E* could raise or lower (just in front of the front disc) by means of a lever and an attached cord that ran over a pulley screwed to the ceiling, exposed the two stimuli simultaneously, and served to control the time of exposure and to maintain a uniform state of adaptation.

To secure equality of tint, we adjusted the fixed light in the lateral direction till a grey disc on the front motor disappeared against its background. We then placed a disc of the same grey on the back motor, and adjusted the variable light in the near-far direction till the hole in the screen went out. Now the grey discs on the two motors received such intensity of illumination that each was the same in tint as the screen. The adjustment of the variable light had to be made at the start of every hour of observation, and sometimes also within the hour; for, although the two lights were on the same circuit, the same settings did not always result in equality of tint. When the colored discs displaced the grey, it happened very rarely that one or other or both of the colors had not the same tint as the screen. Sometimes this difference in tint between color and grey quickly disappeared; sometimes it persisted, and a new adjustment had to be made.

It was soon found that the same position of the variable light would not give equality of tint with every one of the different-sized holes that served as the variable factor. In general, the smaller the hole, the darker in tint did the grey appear through it. This meant that with the smaller holes the light came forward and with the larger it went back; it meant also that the light had its particular position for every one of the variable stimuli. That position had also to be determined at the start of every hour of observation. Matters were, indeed, even more complicated: variable no.4 (the next to the largest, 37 mm. in diam.), which should have given the next to the lightest grey, gave a grey that was almost as dark as that of variable no. 1, the smallest. In the hope that this result might be a function of the particular card used, and not of the size of the opening, we made two other cards with openings of the same size; but all gave the same reversal. We then made two other cards, with openings 2 and 4 mm. greater in diam. than our largest variable. These cards gave a grey lighter in tint than that of card no. 5. We have no explanation of the anomaly to offer.

Since the experiences of R and G had shown the richest chroma in the previous work, we decided to work with these stimuli. We were able to complete the observations on G; these on R suffered for lack of time.

O sat 3.5 m. in front of the screen, with his head held in a head rest. The variable stimuli came in haphazard order with the proper adjustment of the variable light for the particular stimulus. The variables with the R stimulus were 20, 21.5, 23, 24.5, and 26 mm.

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[2]For a general discussion of the film color see M. F. Martin, Film, Surface and Bulky Colors and Their Intermediates, this JOURNAL, 33, 1922, 451-480.

The *O*s were Dr. H. G. Bishop (B), Diana Ginsberg (Gi), and Dr. L. B. Hoisington (H). B and H, members of the department of psychology, were trained *O*s; and Gi, a senior with her major work in psychology, had had a good deal of experience as *O*.[4] The instructions were: "You will be shown, simultaneously, two colors of the same hue. You will judge the relative chroma of the right hand color in terms of the left. If the color experiences show other differences, you may note them; but make your judgment in terms of chroma alone."

Table I gives the limens for the increase of chroma in terms of visual angle. In the case of R, B reported that the experience correlated with the back disc had better chroma than the other with the largest variable, and even with the 18 cm. opening, despite the greater distance of the stimulus. For this reason we were unable to compute a limen for him.

TABLE I

O	Color			
	G		R	
B	40′	5.7″	—	—
Gi	41′	41.9″	26′	36.0″
H	35′	24.5″	23′	4.2″

It should be noted that the judgments were comparative and not absolute as is usual in the determination of an RL. The point of subjective equality could not be determined, as the judgment 'less' was never given. We chose to compute the limen, using Urban's formulae for the method of constant stimulus differences, for the judgment 'greater.' The results would be the same if we had used the distribution of the 'equal' judgments, since the *O*s employed but the two categories of judgment.

The two simultaneous experiences gave rise to two distinct modes of experience, demanding different attitudes. It yet remained to determine whether attitude affects the judgment of relative chroma, or whether the chroma of a color is independent of its mode of experience. As appears later, we must conclude that it is not the same thing to judge the chroma of a perceived color and that of a color that is non-perceptive, as object. The film color differs from the surface color in other characteristics as well, so that it is difficult to say whether the characteristics of an experience are transformed in the perceptive integration, or whether some other of the characteristics of the film color cut across the judgment of chroma. We are, however, certain that in the perceptive experience the attributive aspects themselves get 'perceptualised' separately, whereas in the non-perceptive experience all the attributive aspects come beaten up into an undifferentiated totality. It may be, therefore, that our two alternatives amount, in the end, to one and the same thing: the perception of the color transforms the total non-perceptive experience, and in so doing it changes the chroma along with the other aspects of experience.

That the judgments were not wholly unequivocal is borne out by the fact that the results of Gi show inversions of the first order with both G and R; that those of B contain inversions of the first order with G, while he was unable to make the judgment with R (although the limens of both Gi and H were less for R than for G); and that the results of H contain inversions of the second order. Statements by B and H tend to prove the equivocal nature of the judgment. B reported: "There is an increase in filminess, but I am not sure whether it is chroma." "Even if the chromas are equal, the experiences are not." H on one occasion remarked: "There is something else there than chroma. What this something else is I can not say."

[4] A. J. Rubin acted as *E* when Gi was *O*.

The intruding factor or factors may be due to fixation or to attitude or to a combination of the two. H reported: "If I fixate the screen just at the side of the hole, there is an added perception of transparent bulkiness in the color that I see through the hole. This does not mean that I see the hole and the color separately. It comes as a greater amount of color, but not as a difference in degree of chroma. If I fixate half-way between the two colors, there comes a soft rosy glow. This glow looks very much like greater chroma. I get this in a more pronounced degree with some of the smaller holes. If I look at one color and then at the other, with this large hole (41 mm.), I see no difference in chroma. There is a difference in the apparent depth of the two experiences." And again: "If I fixate the edge of the screen in front of the front disc, the standard, I get a white-veil effect. The color seems to lie behind the white veil, and to be rather decreased in chroma on account of it. If I fixate the disc itself I get none of this. The color as seen through the hole (37 mm.) does change from practically true film to a little way towards bulkiness with a shift of fixation from within the color to any point on the screen. This carries a meaning of more color, and often increases the gloviness which at times comes as greater chroma."

Under certain not well-defined conditions the standard or perceptive color split up, as in the above report from H, into a grey and a red component. B made a like observation. "I took the front disc, my standard, as red as I could see it, and compared it with the hole which I tried to take as red without regard to its filminess. The glowy red of the hole is a redder experience. The other, the standard, is red and greyish. This time I took the red apart from the grey. The glowing character of the color through the hole remains fairly constant."

The film color, especially at the moment of exposure, also presented changes. B further reported that the variable was a washed out red, till filminess came in; and for H the luminous character came with or before the increase in chroma.

In the light of the above discussion, we offer the limens for whatever they may be worth. Whether they prove upon repetition to be approximately correct (as we are disposed ourselves to believe, since our *Os* were trained) is not very important. The main aspect of our work, we believe, is the attempt to get at the problem of the changes in the attributive aspects of experience under various perceptive attitudes. Our results show that this problem is at least not impossible of attack.

In summary, our *Os* were able to judge the relative chroma of a film color when compared with a surface color. In five out of six cases we could compute the limen for the increased chroma of the film color. Under our conditions and with our technique, the factors of chroma, volume and luminosity were bound together in the film color; we could not eliminate one without eliminating the others.

LXIX. A Preliminary Study of the Bourdon Illusion

By B. R. Rubin and H. P. Weld

In a discussion of the "tendency to imagine movement when similar objects appear successively in different positions" Bourdon describes an apparatus for its demonstration. The apparatus consists of a rotating white disk, upon the periphery of which are drawn a number of black figures. These figures are identical in size and shape and are equally distant from one another and from the center of the disk. In front of the rotating disk is placed a screen, in which a slit or window is so cut that the figures,

according to the direction of rotation, may be seen as successively rising or falling. "If", Bourdon says, " the interval between the disappearance of one of the figures and the appearance of the next following is sufficiently short, one notices, in addition to the real movement of the figures, a movement in the opposite direction which is produced every time that a new figure appears in the slit. And, in fact, this second figure succeeds an identical figure which has just disappeared; consequently, one thinks one sees the figure which has just disappeared return to the position occupied by the figure which is now visible."[1] Bourdon does not attempt an explanation of the illusion; and although its conditions are in some respects similar to those of the stroboscopic and tachistoscopic illusions of movement, they are still so different that this illusion cannot be brought offhand into relation with the others. We have thought it advisable, therefore, to undertake its study. Our immediate problem was to determine, first, the optimal and limiting rates of objective movement, for both directions of rotation; to obtain an accurate account of the appearance and behavior of the moving object; and finally, to get a psychological description of the processes in the perception.

Apparatus. Our apparatus consisted of a large disk, 31½ in. in diam., made of heavy white compo-board and mounted on a ball-bearing axis. On this disk were painted 10 black figures, trapezoidal in shape, and measuring 4¼ in. on the top and bottom sides, 3⅛ in. on the side facing the periphery of the disk, and 2¼ in. on the side toward the center. They were placed 2⅛ in. from the periphery and 9⅛ in. from the center of the disk; the distance between them was 5½ in. on the outer side, and 3⅞ in. on the inner side. The disk was rotated behind a screen, with a window, of the same white as the disk. The window was also a trapezoid, the upper and lower sides being 6¼ in., the side toward the periphery 4¾ in., and the side toward the center 3½ in.; the distance between screen and disk was 5 in. When the disk is rotated slowly, *O* sees the black figures rising, if the direction of rotation is clock-wise, and falling, if the direction of rotation is counter clock-wise; when, however, the disk is rotated at certain intermediate speeds, the black figures are frequently seen first to rise and then to fall, when the direction of their objective movement is upward; and by some *O*s, first to fall and then to rise, when the objective movement is downward.[2]

Attached to the back of the screen was a white shutter, covering the entire window, which could be lowered and raised from the side by means of a wooden handle. In the center of the windows was placed a fixation point, which consisted of a knot made in the middle of a length of hair and painted with china white; the hair was stretched vertically across the center of the opening. Although the experiments were performed in daylight, the illumination of screen and disk was increased and kept more nearly constant by a 200-watt nitrogen lamp with a good reflector placed at the side of *O*.

To control the rate of rotation of the disk we used an Edison phonograph motor which, under a pressure of 8 volts, has an approximately constant rate of speed. Between the motor and the disk was a reducing cone, so that by various adjustments of the belts a number of different rates of rotation could be obtained. These rates were calculated from the circumferences of the motor wheel, the disk wheel, and the grooves on the cone, and were checked by a stop-watch. The following rates (times of a single rotation of the disk) were employed: 1, 1.5, 2, 4, 6, 8, 10, 12, 14, 16, and

[1]B. Bourdon, *La perception visuelle de l'espace*, 1902, 193f.

[2]The disk and screen here described were made to Professor Titchener's specifications, for purposes of lecture demonstration, to replace a less accurate model in use since 1905. Cf. E. B. Titchener, *Textbook of Psychology*, 1910, 359.

18 sec.; since there were 10 figures on the disk, a new figure appeared every .1 sec. for the fastest, and every 1.8 sec. for the slowest rate of rotation.

The *O*s were Miss G. K. Adams (A), a graduate student in psychology; Dr. H. G. Bishop (B), Mr. S. Feldman (F), and Professor L. B. Hoisington (H), members of the staff; and Miss Schuster (S), an undergraduate student majoring in psychology.

In determining the optimal rates we gave the following 'meaning' instruction: "A stimulus will be shown you, which will arouse a visual perception of movement. Report the direction of the movement and the nature of the moving object."

The procedure was as follows. *E* first set the apparatus in motion at the desired rate of rotation; then, when the rate was well established, gave the "ready, now" signal; raised the shutter; and exposed the disk for the period of one complete revolution. *O* then gave his report. After *E* had given the 'meaning' instruction for two series,—the first starting from the slowest and ascending to the fastest rate, and the second starting from the fastest and descending to the slowest rate, or conversely,—a 'process' instruction was substituted for it. This read: "You will be shown a stimulus which in previous experiments you have characterized as a downward (an upward) moving black object. Now describe the processes in strictly psychological terms."[3] Series with both modes of instruction were repeated until the reports became consistent for every *O*.

Results. Under the 'meaning' instruction four types of judgment concerning the direction of the perceived movement were given. These were 'up', 'down', 'up and down' and 'no direction.' The perceptions under these categories, when the disk was moving in the clock-wise direction, were in general as follows.

The 'Up' Movement. A black figure, called at various times a 'rectangle,' a 'sector,' a 'wedge,' appeared with a 'jerk' or 'jump' at the bottom, then rose, and finally disappeared at the top of the field. There followed an interval when only the white background was seen. Then another black figure like the first came into view, and crossed the field as before. With the slowest rates all four borders of the figure were sharp and distinct; with faster rates the upper and lower edges became jagged and blurred; with still faster rates the figure became narrower in the vertical dimension, and the upper edge became slightly concave, the lower convex. Ultimately, with the fastest rates of all (1, 1.5 sec.), a flickering gray band crossed the field in the vertical direction; with the slower of these rates transverse bands of gray (two or three of which were always present in the field) were occasionally reported as having an upward motion, but they never entered and left the field.

The 'Down' Movement. With the faster rates at which discrete objects were seen (2, 4 and 6 sec.), something (said at times to be like the 'rectangular' figure and at other times to be identical with it) appeared suddenly at the upper and then quickly dropped to the lower edge of the field, where it as suddenly disappeared. It was never reported as seen in the act of entering and leaving the field. Although, therefore, the trapezoidal figures were objectively rising, they were not in these cases perceived as rising. This is an illusion which is not reported by Bourdon.

The 'Up and Down' Movement. The black figure first appeared at the bottom, rose, and then, with the slower rates, disappeared at the top. But just at the instant of disappearance it, or something similar to it, suddenly

[3] *O* was also asked casually to report direction of movement if it was noted. This request was not incorporated in the typewritten instruction given to the *O* for fear that it might predispose him to an objective attitude.

TABLE I

Showing the Relative Frequencies with which Every Direction of Movement was Perceived by Every *O* at Every Rate: Clock-wise Direction

Direction	Obs.	1	1.5	2	4	6	8	10	12	14	16	18
Up	A			17	14			17	20	33	14	67
	B		17						17	17	17	17
	F	20	20		20	20	40	60	80	100	100	100
	H	14					12.5	37.5	62.5	87.5	100	100
	S		28.5		14	33	71	71	86	86	86	87.5
	Total	6	14	3	10	9	24	37	55	66	64	75
Down	A	17	33	33	71	62.5	57	50	20	17		
	B		17					17				
	F		20	20	20							
	H			33	17							
	S		43	100	28.5	14						
	Total	3	24	37	30	18	12	13	3	3		
Up and Down	A	83	66	50	14	37.5	43	33	60	50	86	33
	B		17	100	100	100	100	83	83	83	83	83
	F	60	60	80	60	80	60	40	20			
	H		40	50	83	100	87.5	62.5	37.5	12.5		
	S	14	14	14	57	57	28	28	14	14	14	12.5
	Total	30	38	57	59	74	64	50	42	31	36	25
No direction	A											
	B	100	50									
	F	20										
	H	86	60	17								
	S	86	14									
	Total	61	24	3								

reappeared at the top and quickly dropped to the bottom, where it was met by the next rising figure. The downward was more rapid than the upward movement: the figure was said to 'stamp down by itself', 'to beat down' (A), to 'flash down' (B), to 'kick down' (F), to 'flit down' (H), to 'pop down' (S).[4] The downward moving body was not well defined throughout its course. H says (rate 8 sec.): "It was a rectangle in the first part of the downward movement, but it faded out and became just something moving;" and again (rate 10) "I do not see any definite object move down: something moves down, and it is taken as the same rectangle." B reports (rates 18 and 16): "It was a pretty poorly marked flash down;" "It was a flash down all the way across the field;" (rate 10) "It is a flash part of the way, and a full sector the rest;" (rate 6) "For very little distance at the top it was a flash, for the rest of the way it was a sector;" and finally, (rate 4) "It was a sector both ways." This, then, is the illusion described by Bourdon.

The 'No Direction' Movement. This type of movement occurred almost exclusively with the two fastest rates 1 and 1.5 sec., and more frequently with the former than with the latter. With the 1 sec. rate the object was a "dark gray circular strip that extended in a vertical direction across the field;" the movement was a 'flicker;' and 3 of the 5 *Os* reported it generally as having no direction. With the 1.5 sec. rate the field was a flicker of narrow black and white bands. H says: "It is hard to say they are objects; they are just streaks of light and dark." The other *Os*, however, refer to them as 'sectors.' B describes the movement as an appearance and disappearance without (spatial) movement; and H also characterizes it as "a disappearance and reappearance." The reappearing streak, however, "may appear further up the field when it has the meaning of moving up." Similar reports were occasionally made also with the 2 sec rate.

In Table I we give the relative frequencies with which these types of movement were perceived at all the different rates of rotation. The horizontal line marked 'Total' gives the relative frequencies with which all the judgments, at the rate marked at the top of the column, fell into the particular category. For example, of all the reports given when the rate was 1 sec. per revolution of the disk, 6% were 'up', 3% were 'down,' 30% were 'up and down,' and 61% were 'no direction.'

The table shows that, in general, the relative frequencies of the 'up' judgments increase as the rate becomes slower. Although rate 18 was not slow enough always to inhibit the illusion for more than two *Os*, 75% of all reports at this rate were 'up;' *i.e.*, accorded with the direction of objective movement. Despite the fact that all *Os* occasionally gave 'down' judgments, only two (A and S) gave them with any degree of consistency, and the total number is smaller than for any other category. These judgments are most frequent with the faster rates at which discrete objects were seen (2 and 4 sec.), and decrease as the rates become slower. The 'up and down' reports are more frequent than any other, and for 3 *Os* occur even at the slowest rate. Their frequency has its maximum at 6 sec. (74%), and decreases on either side of that rate. The judgments of 'no direction' are most frequent with the most rapid rate; they decrease rapidly as the rate becomes slower; and they do not appear beyond 2 sec.

Finally, the table shows marked individual differences. Except at the extreme rates A reports the 'down' more often than the 'up and down' movement. B saw the Bourdon illusion almost invariably, while S gives a figure of only 57% at the optimal rate.

Before considering the content-processes involved in the perception, we shall report briefly the quantitative results of the experiments in which the disk was rotated in the counter clock-wise direction. All other condi-

[4]Bourdon uses the phrase "brusquement redescendre" (*op. cit.*, 193); and, in describing another and similar illusion, speaks of 'saccades' (194).

tions, of course, remained the same as before. Under these circumstances the stimulus object moves downward, and the illusion in Bourdon's sense should first be a downward and then an upward movement. We performed in all 814 experiments: the perception was 'down' in 81%, 'up' in 5.5%, 'down and up' in 10%, and 'no direction' in 3.5%. Two *Os*, F and S, never at any time saw the illusion; of the other 3 *Os* none saw it at the 1 sec. or beyond the 10 sec. rate. A reported it in only 3.6%, and B and H in only 25% of all trials. B, however, observed the illusion in 80% and 94% of the experiments at the rates 2 and 4 sec. respectively. A saw the 'up' movement in 16%, and H in 25% of all experiments. The net result of these experiments with the disk rotated in the counter clock-wise direction is that the Bourdon illusion occurs with a frequency much less than when the disk is rotated in the opposite direction. Some *Os* do not see it at all; and for those who do the range of rates at which it is seen is much narrower.

The Introspective Results. Except for a difference in the spatial aspect of the perceptions, the reports obtained with the disk moving in the two directions were substantially the same. We shall therefore describe the perceptions in the sense of the clock-wise direction. Furthermore, since the 'up and down' movements seen with the most rapid rates (1 and 1.5 sec.) were only vibratory or flicker movements of small extent, and not therefore of the kind mentioned by Bourdon, we shall disregard the reports of them. Finally, since the reports will be more easily understood if we give general accounts of the processes involved in the perceptions of the 'up' and of the 'down' movements before we describe the Bourdon illusion, we shall take them in this order.

The 'Up' Movement. H "A rectangular-shaped extent of black quality and definite contours came into the field below and spread upward across the field and disappeared. As it left the field its lower edge was a bit fuzzy, shading off through gray to white. The rate of progression was not quite uniform; it spread rather quickly at first, then rose at a uniform rate across the field to the top, and spread a little quicker. The black was followed by a white quality, and then the black appeared again as in the beginning." A and B further describe the black as a surface, and A says that the white between the successive blacks is "more silvery and misty" than the white of the background.

The 'Down' Movement. A "Black on white above the center of the field; then something flashy, I cannot report its extent or tint for it was so quick, but it does not belong to the black or the white, it rather extends over both at the same time. I changed to the meaning attitude and the 'down' was very strong; it seemed as if the whole black were moving. I then changed back to the process attitude and saw grey where I thought it was black." H "A black quality sharply defined on the right and left sides appeared near the top of the field. The black quality then progressed downwards. As it approached the lower part of the field it faded into a lighter gray; then it darkened a little more." "A stretch of black at the top of the field. The lower edge was very fuzzy, practically white at the bottom; the denser black part was a little more extended in the vertical than in the horizontal dimension. Then extension downward, a spread of the black or, better, dark gray quality down pretty well across the field. This gets lighter as it approaches the bottom, where it is a fairly light gray." None of the other *Os* reported this perception in the 'process' series.

The 'Up and Down' Movement. A "Black first at the bottom of the field; moved up to the top. Then a gray flash which, in a way, went from top to bottom; this was of short duration. Black then appeared at the bottom; it expanded and expanded until it reached the top; then gray flash again from top to bottom." B "Lots of white between black sectors. The

lower edge of the sector at the top is going out as the upper edge of the sector at the bottom is coming into the field. Just as the top sector goes out there is a flash of something which starts at the bottom of the top sector. It is a very elusive experience. The flash is slightly grayish, practically like a curtain of gray, a diaphanous gray; it lies before the white, comes in with a snap, and ends pretty suddenly." H "A broad spread of black quality quite sharply outlined on the right and left sides and a little fuzzy on the lower edge spread up from the lower part of the field to the top; then an extension of gray downward. In quality it was a gray black, and it faded out to light gray at the bottom. It spread pretty well over the entire field; the black itself spread down a little." These reports, selected at random from a large number, are perhaps sufficient to show that the processes involved in the Bourdon illusion are a combination of those in our 'up' and 'down' perceptions. The characteristic process in the downward movement is a gray which seems to grow out of the lower sector of the disappearing upward moving black and to spread downward across the field. In quality it is generally described as a dark gray, lighter than the black; H says that its quality is not uniform, and that it shades off into a lighter gray at the bottom of the field. It is also said to be 'transparent', 'vaporous', 'misty', 'foggy', and the like. All three of the *Os* who describe it agree that it has a short duration. A and B habitually refer to it as a 'flash of gray', and occasionally both characterized it further as 'lively'. H, however, purposely avoided the word 'flash' because that meant for him an extension solely in time; he employed instead the term 'spread' or 'extension' of gray. Frequently he reported difference in amount of 'spread', as, for example: 'it was not uniform in extent throughout the exposure period; sometimes it was clear across the field, at others not half the way." He used these terms 'spread' and 'extension' without prejudice to the question of spatial movement of the gray. He once reported: "a total spread over almost all the field; the spreading seems to be all the movement there is, but I do not know whether it is time or space." To this question B gives no answer, and A gives two,—one of which is probably, and the other flatly negative: "There was not much change in its position or amount; I see it, and then I don't;" "It did not really move in any direction nor was it still."

Discussion of Results. There would seem to be no doubt that the downward phase with clock-wise movement of the disk is, on the side of content-process, a gray quality of short duration which differs phenomenologically from both the black of the figures and the white of the background; it is a 'lively', 'flashy' gray. In these respects it is apparently the same as the gray reported by Dimmick; and the Bourdon illusion seems therefore psychologically to be the same as the tachistoscopic illusion.[5] There is, however, a possible difference. Dimmick's gray did not change its position spatially, whereas our gray may have moved; our results, despite A's negative report, are inconclusive on this point. It would not be surprising, however, to find a difference of this sort where the conditions of illusion are different: in Dimmick's experiment there is no objective movement, while in ours the stimuli are moving across the visual field.

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[5]F. L. Dimmick, this JOURNAL, 31, 1920, 317. In his lectures of the last three years Professor Titchener has employed the Bourdon illusion to illustrate Dimmick's result. The demonstrational disk is given a sharp swing (in the clock-wise direction) and is allowed to die down. As the movement slows, the illusion of down-stamping movement appears; now this and now that member of the audience, within a brief period of time, 'gets' the illusion; and after it has established itself there is still plenty of time to observe the gray smudge. The demonstration requires only a few minutes, and is reliable. A disk of the dimensions given in the text is large enough for an audience of two hundred.

Both the characterization and the psychological description of our 'down' movement indicate that this perception is simply the downward phase of the Bourdon illusion; for some reason the upward phase is not seen. A, the *O* who most frequently made this report, suggests the reason. In her 'meaning' reports she says: "Movement was up and down in a continuous string of movements; the downward movements were more pronounced." "Black figures with smooth edges stepped down very quickly; the downward movement forces itself clear, the fall is so rapid." "Black on white beating down; movement of up was only secondary and did not attract attention." These reports were given when the *O* was obscurely aware of the upward movement; the lively gray flash coming suddenly caught the attention, and the more slowly moving surface black dropped to the lower level of consciousness. In the extreme case the upward movement does not get reported at all.

The individual differences in the frequency with which our *O*s saw the illusion mean that there is some condition which we have not controlled. Under our conditions the exposure time (the time of a single rotation of the disk) at the more rapid rates was very short; it may be that some *O*s would have had the illusion more frequently with a longer exposure time. Furthermore, in the course of our experimentation we found that even with a fixation point in the field steady fixation is difficult; our *O*s frequently reported that their eyes moved. We found also that a constant distribution of attention over the entire field of observation is difficult to maintain since the moving figure tends to demand attention. Finally, the fact that the illusion appeared with a frequency much greater when the disk was rotated in the clock-wise than when it turned in the counter clock-wise direction indicates that some more fundamental disposition or attitude was at work. Some item or items of this list may have been responsible for individual variation.

Summary. The Bourdon illusion appears more frequently and over a wider range of rates of succession of the figures when the disk is rotated in the clock-wise than when it is rotated in the counter clock-wise direction. With the former the range is from 0.2 sec. to about 2.0 sec., and the optimal rate is 0.6 sec.; with the latter the range is from 0.2 sec. to 1.0 sec., and the optimal rate for the two *O*s who had the illusion most frequently was 0.4 sec.

The downward movement (with clock-wise rotation), the illusory part of the Bourdon illusion, has as psychological correlate a filmy and lively gray which is not given objectively, and which is phenomenologically different from the black figures and the white background of the disk. With allowance made for differences in objective conditions, this gray is apparently the same as that reported by Dimmick as the correlate of the phi-phenomenon.

Individual differences in the frequency of the illusion suggest the action of conditions not yet controlled.

BOOK REVIEWS

Handbuch der vergleichenden Psychologie. Herausgegeben von G. KAFKA. Bd. I.: *Die Entwicklungsstufen des Seelenlebens*, pp. vi.+526. Bd. II.: *Die Funktionen des normalen Seelenlebens*, pp. vi.+513. Bd. III.: *Die Funktionen des abnormen Seelenlebens*, pp. v.+515. Verlag von E. Reinhardt in München, 1922.

The brief Introduction which opens this comprehensive handbook has two purposes,—to explain and justify the titular use of the term 'comparative,' and to explain and excuse the actual plan and contents of the three volumes.

Every science, the Editor tells us, passes through three stages of development. It begins with hypotheses. It then seeks to test its hypotheses by applying them, for the most part deductively, to facts of observation; and the sounder the hypotheses turn out to be, the more surely do they lead to the discovery among the facts of special uniformities, of which they themselves in their generality had been unable to take account. As these local laws increase in number, the science feels the need, thirdly, of bringing them all together into uniformities of a higher order.

The three genetic stages are paralleled by three forms of established science. Every science, in its first period, seeks to group its facts under laws of the most general kind. It then passes into a period of specialisation, in which theoretical inquiry is restricted to special questions; in this period an applied science is likely to grow up side by side with pure science. In a third period it aims to supplement the general and special laws of the preceding periods by setting up general laws of the second order, which obtain between the facts and laws of the special fields of the second period; and it is in the work of this third period that it becomes, in the narrower sense of the term, comparative.

Psychology is no exception to these rules. The first period of scientific psychology, the period of Lotze, Bain, James, Spencer, Wundt, Ebbinghaus, is predominantly speculative. We find, no doubt, a certain amount of specialised and even of comparative investigation,—the work of Lazarus and Steinthal on social psychology, Waitz' Anthropology. On the whole, however, the science is directed upon general uniformities. The second period is a period of specialisation: psychologists are students of sensation, of feeling, of will, of attention, of memory, of the thought-processes, of psychological aesthetics, of social psychology, of applied psychology, of pathological psychology. Wundt could lay the foundations of his system by first-hand work in all departments of psychology. Nothing of that sort is possible to the younger generation, who are bound to specialise; though they are resigning themselves too easily to the limitations of specialised thinking. All the greater, then, is the present need of a comparative psychology, whose task it shall be "by means of the categories furnished by general psychology to group the results of special psychology into empirical laws," laws too special for general psychology and too general to fall within the competence of the special psychologies.

So much on the formal side; we have now to attempt a material definition of this comparative psychology. Current specialisation has taken four principal directions: those of genetic psychology, social psychology, pathopsychology, and special psychology in the narrower sense (psychology of sensation, feeling, will, etc.). Only the first three of these psychologies can contribute to comparative psychology. We find an analogy in biology. It is possible to investigate the phenomena of organic life solely from the

physical point of view; we then have a physiological mechanics, a physiological chemistry, a physiological electricity, etc. These are, of course, special fields; they belong, nevertheless, not to the special biologies that can serve as basis for a comparative biology, but to general physiology; and they take that place for the simple reason that they do not operate with the specifically biological 'function.' So it is, then, with psychology. What we have called 'special psychology in the narrower sense,' the psychology of 'faculties' taken in abstraction from the concrete life of mind, belongs properly to the general psychology of the first period; the other three psychologies, on the contrary, deal with the actual mental functions. Comparative psychology itself is thus primarily a biological psychology, and may accordingly be defined afresh as the science of the relations between the individual functions of the mental life.

The concept of function enables us, further, to bring out the systematic relationship of genetic psychology, social psychology and pathopsychology. Function implies norm; and we may therefore discard the loose nomenclature of 'social psychology' in favour of 'psychology of the normal functions of consciousness.' What, in fact, distinguishes 'social' from 'individual' psychology is not the collective as distinct from the particular object of observation, but rather the direction upon concrete mental 'functions' as distinct from abstract mental 'forces.' To be sure, the amended definition robs social psychology of certain genetic and abnormal phenomena (mental life of primitive peoples, cannibalism); but these find their proper place in the correlated genetic psychology and pathopsychology. And, finally, the restriction of social psychology to the over-individual effects of human environment is wholly unsystematic; for, on the one hand, all mental functions whatever are more or less affected by social influences, and on the other hand the definition draws an artificial line of separation between genetic stages and abnormalities whose conditioning is primarily social and those whose conditioning is primarily individual.

We are left, then, with the three coordinate psychologies: the psychology of the normal functions of consciousness, the psychology of the stages of development which these functions pass through (genetic psychology), and the psychology of the deviations of the same functions from their norm (pathopsychology). Here is the threefold basis of a comparative psychology in the sense of the handbook.

So far I have followed the Editor. I now break off to give a table of the contents of the three volumes.

Vol. I. The Stages of Development of the Mental Life

I. Animal Psychology, by G. Kafka (Munich), pp. 11-144
II. Psychology of Primitive Man, by R. Thurnwald (Halle), pp. 147-320
III. Child Psychology, by F. Giese (Halle), pp. 323-518

Vol. II. The Functions of the Normal Mental Life

I. Psychology of Language, by H. Gutzmann (Berlin), pp. 3-90
II. Psychology of Religion, by G. Runze (Berlin), pp. 93-180
III. Psychology of the Arts, by R. Müller-Freienfels (Berlin), pp. 183-336
IV. Psychology of Society, by A. Fischer (Munich), pp. 339-456
V. Psychology of Vocations, by O. Lipmann (Berlin), pp. 459-508

Vol. III. The Functions of the Abnormal Mental Life

I. Psychology of the Abnormal, by H. W. Gruhle (Heidelberg), pp. 3-151
II. Psychology of the Criminal, by M. H. Göring (Giessen), pp. 155-229
III. Psychology of Dream, by S. de Sanctis (Rome), pp. 233-329
IV. Psychology of the Sexual Life, by R. Allers (Vienna), pp. 333-506

The second part of the Introduction, as I said above, explains and, where necessary, excuses the plan and contents of the book. The Editor finds the arrangement of vol. I. satisfactory, and, for the present, exhaustive; someday a section on Plant Psychology may be in order. He is less satisfied with vol. II. Language, he remarks, can lay claim to special treatment on account of its threefold rôle as product, mediator and developer of psychical functions. The Psychology of Society, however, should have had more space; the relation of man to man is at least as complex, at least as finely differentiated, as his relation to God and the world. If, nevertheless, the single section stands over against the three sections devoted to Religion, the Arts, and the Vocations, the reason is historical: the psychology of society has in the past been conceived, too narrowly, as the psychology of custom, and custom has been set directly alongside of language, myth, etc.; so that the free course of investigation has been checked. This second volume should have contained, further, a section on the Psychology of the Sciences, ground for which has been broken by the studies of Groos and Jaspers; the Editor was unable to find a psychologist willing to undertake it. Lastly, the Introduction is at pains to defend the inclusion in vol. III. of the section on the Psychology of Sex. The sexual function as such, we are told, is altogether normal; but its activity may condition, both in the animals and more especially in man, disturbances of other biological functions, which themselves approximate the pathological, and which may further be the cause of a strictly pathological *habitus*; it is therefore from the point of view of aetiology that the function takes its place among mental abnormalities.

All these are matters of detail, though the detail is in some cases important. The main criticism which the Editor foresees, and the objective justice of which he admits, is that the handbook is, after all, not a handbook of comparative psychology; it is a collection of special monographs; and the work of comparison, the aim and justification of the whole enterprise, is therefore left to be done for himself by the reader. The excuse offered is that nothing else than a book of this sort is now possible, and that the writing of the handbook will at any rate bring our need of a comparative psychology into clear daylight, will arouse and reinforce interest in its particular problems, and—by showing where the work of the specialist is still incomplete—will serve to strengthen the foundations upon which it must ultimately rest. To all of which one may say Amen, while yet one wishes that the Editor had had the courage of his insight, and had taken upon himself something of the labour and responsibility that he now hands over to his readers.

Meantime, the specialist remains in possession of the field. I hope that we shall be able, in later numbers of the JOURNAL, to publish reviews by competent hands of the special sections of the Handbook. The names of the authors are a sufficient guarantee of good work, and the Editor deserves our grateful thanks for having persuaded them to cooperation in what is undoubtedly a valuable reference-book. Whether his teleological position is necessary, and how far teleological modes of inquiry can be scientifically fruitful, are questions that can best be raised in connection with the contents of the special sections.

E. B. T.

Experiments in Psychology. By WILLIAM S. FOSTER. New York, Holt and Co., 1923. pp. x., 309.

Whether the student's ultimate interest be psychology or something else, it is desirable that he should have at least an elementary knowledge of scientific method. In his *Experiments in Psychology* Foster has arranged a laboratory course which is designed as a "general introduction to scientific method" and at the same time as a "partial basis for more advanced work

in experimental psychology." The book is made up of two Parts and an Appendix. Part I consists of introductory matter, Part II of the experiments, while the Appendix gives directions for the construction of much of the simpler apparatus. There is no index.

The first chapter in Part I discusses the relation of the psychological experiment to scientific psychology, introducing a certain amount of controversial matter,—always a questionable practice in an elementary textbook. The next two chapters deal with the conduct of experimentation and with graphic representation, and are explicit, simple and well written. They should do much toward getting the class started right, and will lighten the burden of the instructor. The last two chapters take up representative values (including central tendency), measures of variability, and the reliability of comparisons of representative values. They discuss both the theory and, by the use of concrete illustrations, the methods of obtaining the values in question. These chapters should give the student a good idea how to work out the desired results; but, it is to be feared, will aid him little in understanding what they mean. For this the instructor must use supplementary material.

The experiments of Part II have been selected with the idea of including "methods of measurement and control which are characteristic of scientific procedure in general." Some are individual, the pupils working in pairs; in others the instructor is the experimenter (E) and all members of the class are the observers or subjects (S's); still others are demonstrational, the pupils taking notes on what they observe. They include such topics as space-perception, Weber's Law, reflex action, reaction-time, learning, taste and smell, dermal sensations, vision, correlation, attention, memory, association and judgment. The presentation of each experiment has three main parts: (1) an introductory statement that treats of the theory involved; (2) the experiment proper; (3) results and questions. The questions are excellent for bringing out the principles underlying the experiment and for impressing them on the mind of the student. Introspection is made use of at times but is not emphasized. The results are usually quantitative as well as qualitative.

The experiments seem to the reviewer well chosen, clearly explained, and workable with large classes. Should the work reach a revised edition a freer use of illustrations both of apparatus and for the tabular arrangement of results would perhaps be advisable, and would still further lighten the burden of the instructor. The book in its present form will, however, be of great use to instructors who deal with large classes, especially to those who aim to emphasize scientific method rather than to open the door for systematic psychology.

Clark University MILES A. TINKER

The Psychology of Power, by J. A. HADFIELD. (Originally published in a volume entitled *The Spirit*, edited by Canon B. H. Streeter.) New York, The Macmillan Company, 1923. pp. vi, 54. Price 75c.

In this little book Captain Hadfield proposes, "in the first place, to produce evidence of the existence of resources of power normally untapped; secondly, [to] show that these are psychic rather than physical in character; and, after discussing their relation to the instinctive emotions and the will, . . . [to] consider the means by which they can be made available" (4). To carry out his first aim he points to the many examples in his own experience with cases at the Ashhurst Neurological War Hospital, Oxford, of which the following one is typical: "A corporal, whose courage won the V.C., was for several days cut off from our troops, was exposed the whole time to bombardment (subsisting meanwhile on the barest rations), and yet, in spite of the awful strain, he came out feeling cheerful, elated, and

without fatigue. Several men with him had the endurance to pass through the same experience, but to the end were exhausted and broke down. The corporal had evidently discovered sources of power which were not exhausted by the terrible strain he underwent, but provided an ample resupply" (6).

Captain Hadfield thinks that Mosso's experiments in fatigue, which show that the *mind* fatigues much before the body, are proof of his contention that these "untapped resources" are psychic. He arrives at the same conclusion from his own experiments at the Hospital, which show that when, "under hypnosis, I had given the men the idea that they were very weak, the average grip was only 29 lbs., one of them, a prizefighter, remarking that his arm felt 'tiny, just like a baby's.' My suggestions of strength produced an average grip of 142 lbs. as against 101 lbs. which was the best they could do in their normal waking condition. A second test, measured by the time occupied in holding out a weight, gave similar results. In brief, when I suggested 'weakness', the full flood of energy was checked and the men were capable of only one-third of their normal strength, whereas by suggestion of 'strength' latent powers were liberated and their normal strength increased by half as much again" (11-12).

The application of the above discovery led to the discussion of the relation of will to instincts, because, in dealing with neurasthenics, Captain Hadfield noticed that the patient may be willing to get rid of his obsession but, at the same time, may be actually unable to do so. At this point Captain Hadfield is at issue with M. Coué. As he says, the "energies which give the driving force to our lives are not derived from the will, but from another source; they will be found to have their origin in the instinctive emotions" (22). "Instinctive emotions", for Catain Hadfield, are the same as for McDougall, from whom he is constantly borrowing ideas (23). At this point, especially in his unquestioning reliance on the old fashioned dualism of will and emotion, Captain Hadfield is not at all clear, particularly in his discussion of the relation of the "instinctive emotions" to will. For example: "The instinctive emotions give driving force to the will and put life into great ideas, and, being liberated like the winds from the cave of Aeolus, burst forth, either to do their work of destruction or, if rightly controlled, to speed us, with full-bellied sail, on the voyage to the harbour of our destiny" (23). What seems clear, however, is that a neurasthenic person must have confidence and self-assurance of his power before he is able to use it to his advantage. Therefore Captain Hadfield asks the question, why it is that in "men paralysed in arms and legs . . . the will was quite impotent to restore to health: yet there was nothing organically wrong with such patients. Why, then, were they paralysed? Why did not their strenuous efforts enable them to walk? Because they believed they could not move their limbs. They could say 'I will', but they had not learnt to say 'I can' " (40). What, then, is necessary to gain that all important power—confidence? "Confidence removes this paralysis and turns belief into a mighty impluse to act. It fills men with the strength which makes the soul master of its fate" (40). This, of course, gives the author an opportunity to argue for the ever-present need for faith—even in normal life—and that the reader finds to be, in the course of the book, the faith supplied by the "Christian's confidence and hope" (52). Captain Hadfield is really beyond the realm of scientific imagination in this part of the book, when he writes in the spirit of the following: "Nature is economic in her gifts: she will not give strength to those who will not expend it. These remain uninspiring and uninspired. She is lavish in her gifts to those who will use them, and especially to those who devote them to nature's altruistic ends, for such ends harmonise the soul" (41). "In some cases I have attempted to cure nervous patients with suggestions of quietness and confidence, but without success until I have

linked these suggestions on to that faith in the power of God which is the substance of the Christian's confidence and hope. Then the patient has become strong" (52).

The little book has, however, an intrinsic value, being suggestive to the students of psychotherapy, because it contains cases derived from the actual experience of a field worker. The interpretation, we fear, is confusing; it is beyond the realm of science as we know it to-day.

Northwestern University A. J. SNOW

The Unadjusted Girl. By WILLIAM I. THOMAS. Boston, Little Brown & Co., 1923, pp. xvii+261.

While this book is sociological in general content, there are, in its basic approach, some important implications for psychology, especially those phases dealing with educational and social matter. The author, in developing his psychological foundations for understanding the conduct of delinquent women, presents a schema of wishes as the fundamental motives of human behavior. These are: the desire for new experience; the desire for security; the desire for intimate, face-to-face response; and the desire for recognition. While this particular classification may be criticised, although it agrees fairly closely with Dunlap's, it is important to note that we have here a social-psychological treatment which is not founded upon a plan of theoretical instincts in the manner of McDougall and others. In fact, the author has long since broken away from attempting to write his social psychology in terms of innate tendencies. Rather he has emphasized for many years the importance of desires and social attitudes. These are concrete data, around which we may construct a psychology of social life.

Likewise for education, it is time we were giving up the futile attempt to segregate by statistical methods the "original nature of man" from the acquired. A better idea would be to trace genetically the development of deep-seated ideas and mental sets, with their affective and emotional accompaniments. A lead in this direction could be taken from Thomas' book and from his other writings as well.

While Thomas took his notion of the wishes from Freud, even before Freud was known in this country, he never fell into the mystical particularism prevalent in psychoanalytic camps. He made his wishes much wider than those of sex alone; and furthermore in his present treatment he has begun to see the important implications for social psychology of the work of Pavlov and Watson on the conditioned reflex. We still await, however, some one who will connect up systematically the contributions of Pavlov and Krasnogorski, on the one hand, and those of Sherrington and Watson on the other, with the work of Freud, Adler and Jung. When this day arrives, much of the present misunderstanding and mist will be cleared away from these allied standpoints.

In a foreword to this book Ethel S. Dummer, who has done much for the development of the juvenile courts of this country, writes one of the most succinct reviews of the foremost thought of the day upon women and delinquency that the reviewer has read.

University of Oregon KIMBALL YOUNG

Mind and Heredity. By VERNON L. KELLOGG. Princeton University Press, 1923, pp. v + 108.

This little book is a series of ten popular lectures delivered on the Louis Clark Vanuxem Foundation at Princeton University by the well known Permanent Secretary of the National Research Council. The first three or four constitute the best brief popular introduction to the problems of

Comparative Psychology known to the reviewer, and later chapters give an almost equally excellent account of the scope and limitations of mental testing. The main purpose of the course, however, is the examination of certain pressing social and national problems—education, immigration, communism, and democracy—in the light of biology and of the writer's recent experiences in Soviet Russia. It is all interesting reading.

E. C. S.

Intelligence Testing. By RUDOLH PINTNER. New York, Henry Holt and Company, 1923. pp. 406. $2.50.

This book is designed as a college text and as a guide for teachers interested in mental testing. Part I is largely historical, with a chapter on the concept of "general intelligence;" Part II describes the methods of testing; and Part III gives a brief summary of the results so far obtained.

Binet is the focal point of the testing movement. Two influences shaped his work: on the practical side, the need of more exact tests for the feebleminded, the deaf, delinquents and the insane; and, on the side of technique, the beginnings already made in experimental psychology, eugenics, differential psychology and anthropology. Here the author does not, perhaps, give experimental psychology and anthropology all the credit due. After Binet, a critical trend led to revisions of his method and to the construction of performance-scales and group-tests, while practical applications continued to be made in ever wider and wider fields.

For "general intelligence" the author adopts Stern's definition: "the ability of the organism to adjust adequately to new situations." Spearman's two-factor theory has stimulated discussion which has clarified the concept; but the hypothesis of specific abilities has received a warmer welcome in many quarters. Nevertheless, for practical purposes "the concept of general intelligence as shown in the empirical requirements of an intelligence test as we know it today" is more important. The modes of expressing intelligence ratings are evaluated and subjected to valuable criticism.

In the Part on methods the different Binet scales are described rather fully. The American revisions are presented with special attention to the Stanford Revision, because of its importance and general use. The various performance-scales are next considered. Types of material common to group-tests, to which the mental testing in the army gave a great impetus, are then taken up, and several of the more common are briefly noted. But to obtain a satisfactory knowledge of any test, especially of any group-test, the clinical psychologists must supplement the information here given by much practice with the test itself.

In the third Part, dealing with results, the material is taken up under such headings as the feebleminded, the soldier, the school child, the college student, the delinquent, the negro, and the foreign-born. The summing up is factual and objective, the author seldom venturing to generalize or speculate. A final chapter deals with the inheritance of intelligence. The section as a whole is the best of its kind that we know; but its chief merit, as that of the book, is its usefulness as a guide for wider reading in the field of testing.

Clark University MILES A. TINKER

Human Character. By HUGH ELLIOT. London and New York, Longmans, Green and Co., 1922, pp. xvi, 272.

The author believes that a proper approach to the understanding of the human mind is to be made in no other way than through a knowledge of the emotions. Somewhat like Rousseau, he sees "the world of human life

governed, not by reason, but by passion, emotion, and sentiment." This contention is supported by an array of behavioristic and introspective evidence. Setting out with three major emotions,—egoism, love, and social or moral feeling,—the author attempts therewith to explain action, suggestion, belief, thought, genius, vice and crime. Egoism makes for the survival of the individual; love for the perpetuation of the species; social or moral feeling for the maintenance of society.

Elliot does not insist upon hairsplitting when it comes to a matter of terminology. Instinct and emotion are for him one and the same thing. The text is profusely sprinkled with gems from the great poets. In fact, the reader is led to suppose that Goethe and Shakespeare knew vastly more about the human mind than do the academic psychologists of to-day.

West Virginia Collegiate Institute F. C. SUMNER

Psychology: The Science of Human Behavior. By ROBERT CHENAULT GIVLER. New York and London, Harper and Brothers, 1921. Pp. 382.

The subtitle is the key to the contents of the book. Although the author speaks of a mechanistic psychology, the point of view is overtly that of a teleological behaviorism. "The brain is the great central telephone exchange whereby both vital and mental functions—life and thought —are regulated. It is the instrument whereby man's many energies may be guided and combined and brought into harmonious and profitable relations with one another." Mind is action and "*mental activity is a combination of physical, chemical and electrical energies developed by the nerves, muscles and glands of the human organism.*" These special mental energies always express themselves in actions, "some of which we call sense-perceptions, some we call thinking, some we call feeling, some we call skill, and so on throughout the long list of mental powers and functions." The problem assigned to psychology indicates the scope of the book. "For just as a scientific study of the physical world and of the chemical world and of the world of mechanics and geology arose out of an ambition to control and manipulate the physical environment, so the chief motive underlying the scientific study of psychology was a desire to employ the mind to the greatest possible advantage." The method of attack is by induction;"the above case of proof is typical of all scientific experimentation, and is called proof by induction."

This general orientation indicates fairly accurately what the headings as well as the contents of the 36 separate chapters must be. After the introductory chapters, the plan of procedure is, generally, first to reduce the particular mode of mind to kinaesthesis, controlled and guided by the great kinetic system; secondly to explain in neural terms; and thirdly to offer suggestions for further useful development. The treatment of attention is typical. "The mental activity known as attention is, like all other mental processes, a sensorimotor response to stimulation." "The distribution of the environment into focus and margin, foreground and background, is to be found in the principle of the *final common path.*" We are able to maintain any focus continuously, for "the kinetic system regulates the supply of energy to one set of muscles rather than to another, so that a response once begun is continuously maintained." The particular focal point depends on interests. Therefore, if we are to develop the powers of attention, we must develop our interests, a point missed by most authors of books on the "power of concentration."

The book, which is a combination of functional, dynamic and behavioristic psychology and good advice, contains nothing essentially new, although the remarks about hypnosis, will, mental hygiene and industrial adjustments, just as the above about books on attention, are sane and healthy enough to bear repetition. Since in the author's belief all sensa-

tions reduce, in last analysis, to kinaesthesis, it is apparent why a continuous series of sensations or ideas is necessary to maintain attention; for, after all, "environment drives the brain, and the brain drives the various organs of the body." But it is not so apparent how pleasure, which is not a sensation but a quality that attaches to any sensation, or how retinal disparity, as such, reduces to behavioristic terms: both seem to carry meaning in their own right; pleasure signifies a state of the kinetic system rather than is a state of that system, and the cues from the double images of binocular vision are subconscious. It also seems a little far-fetched to explain hypnotic suggestion in terms of the conditioned reflex, especially since we find nothing about the conditioned reflex in the previous chapter on suggestion. Here we are told that "autosuggestion is the basis of all suggestibility. What others say and do excites imitative tendencies in us only so far as we have subconscious leanings in that direction." I am free to confess that I do not know what the subconscious is; it may be a matter of conditioned reflexes.

The style and atmosphere of the book suit the contents very well. We deplore the lack of an index; the frequent failure, except when quoting from medical men, to give credit; the general lack of references; and in some cases, where quotation is almost verbatim, the lack of quotation marks.

For those who believe that science in general and psychology in particular have as their rightful subject the study of human conduct in all its various aspects, the book will be of interest; for those who believe that the province of psychology is the study of experience in some one or other of its various aspects, the book has no appeal.

Cornell University L. B. HOISINGTON

Introduction to the Use of Standard Tests. By SIDNEY L. PRESSEY and LUELLA COLE PRESSEY. World Book Company, Yonkers on Hudson, N. Y., 1922, pp. 263.

In preparing this manual the authors have sought to be comprehensive rather than to present an exhaustive treatment of any one phase of the testing question. Their avowed purpose is to produce a book for the use of teachers, supervisors, and superintendents not especially trained in the use of modern educational measurement methods. Questions of theory have been left undiscussed, and attention is given to the use of tests as they appear on the market.

The book is divided into three sections. In the first section, "How to Use Tests", the first chapter, entitled "What is a Test", discusses the difference between the modern Test and the "Examination", pointing out the advantages of each. The specific nature of the information gained from the use of a standardized test, the saving in time, and the possibility of a comparison between individuals and groups resulting from a standardized procedure in presenting and scoring standardized material, are the principal advantages of the "Test".

Chapter 2 is given over to "School Problems and Tests". These problems are presented in three separate groups: the problem of instruction, which is primarily of interest to the teacher; that of supervision, which concerns the supervisor or principal; and, finally, the problem of administration, which belongs to the superintendent. The applications of the different types of tests to these problems are indicated. In this connection the authors make a plea against testing merely for the sake of testing, and urge that the tester always have some practical problem in mind.

The third chapter, "The Common Sense of Statistics", is especially worth-while. It stresses the fact that statistical treatment is only "refined

common-sense" used in finding "convenient ways of handling data." Each of the statistical methods ordinarily employed in educational measurement is taken up from this point of view with particular emphasis placed on the value of tabulations. The general thesis of the chapter seems to be that statistical treatment should ultimately yield not simply a median or statement of relationship, but also some idea of what must be done to solve the problem at hand.

Chapter 4, on "The Use and Misuse of Tests", is devoted to pointing out certain common errors and their attendant results.

Section 2 of the book includes chapters 5 to 10. It deals with the tests used in measuring achievement in school subjects. Under the headings "General Nature," "Practicability," and "Use" the section discusses "Tests in Content Subjects: History and Geography," "The Measurement of Ability in Written English," "Tests in Reading," "The Measurement of Handwriting," and "Tests in School Subjects," giving a chapter to each.

Section 3 on "Tests of General Ability" points out the need for such tests in supplementing achievement tests and describes a number of tests of this type (Chapter 11). More specific treatment of the use of these tests in problems of vocational guidance, the study of disciplinary cases, and the comparing of classes and school systems as to "pupil material" is given in the final chapter of this section (Chapter 12).

The concluding section bears the heading "Important General Principles Regarding Tests." "How Tests are Made," "The Testing Program," and "How to Make the Testing Program Worthwhile" are chapters particularly suited to the educational executive who lacks an intensive training in standard measurements. They are designed primarily to prevent the waste of time that comes from "testing for testing's sake."

There are four appendices which supplement the contents of the book. Appendix A gives detailed instructions for "Finding the Median for Large Distributions;" Appendix B covers "Tests and the Diagnosis of Feeble-Mindedness;" Appendix C gives "Suggestions for Further Reading;" and Appendix D is a list of "References for Further Reading."

A further convenience is a glossary of technical terms. In the book these terms are indicated, so that the reader may know whether or not an unfamiliar word will be found in the glossary.

Cornell University F. L. Bixby

PSYCHOLOGICAL PERIODICALS

L'année psychologique, vingt-deuxième année (1920-1921). Ed. by H. PIÉRON. Paris, Félix Alcan, 1922. pp. xii, 608. Price 40 fr.

The plan of the 22nd volume of the *Année* is the same as that of its immediate predecessors: a few original papers, notes and reviews, digests of psychological literature, and the year's chronicle. The *Analyses bibliographiques*, which occupy more than half the volume, cover the two years 1920 and 1921. This section of the *Année* is therefore, for the first time since the war, brought up to date. Henceforth the digests will be limited to the books and papers that belong properly to the year's review. In his choice of articles and books for report, the editor has attempted to do justice to the entire range of contemporary psychology. He thinks it worth while, however, to state that in the multitude of Freudian articles he has found nothing useful. "While the fundamental concepts [of Freudian psychology] merit examination and discussion, the concrete observations and particular applications of its method coming from believers without criticism and impregnated with subjectivity are of no general value; the collection of sexual interpretations is like a collection of brilliantly colored butterflies."

The first of the *mémoires originaux* is an investigation of visual qualities beyond the central region of the retina (*Les sensations visuelles élémentaires en dehors de la région centrale de la rétine*) by M. Foucault. Then follow investigations of the behavior of various species of spiders (*Recherches expérimentales sur le comportment de diverses araignées*) by É. Rabaud; of the law which relates the duration of latent intervals to the intensity of stimulus (*Nouvelles recherches sur l'analyse du temps de latence sensorielle et sur la loi qui relie ce temps à l'intensité de l'excitation*) by H. Piéron; of motor reactions in emotional crises (*Les réactions motrices dans les crises dues à l'émotion*) by H. Wallon; of meaningless tactile impressions (*A la recherche d'une sensation tactile pure*) by J. Philippe; and of the mental functions of children of school age (*Recherches sur les fonctions mentales de l'enfant à l'âge scolaire*) by J. Abramson. There are, then, three papers in experimental psychology, one in animal, one in child psychology, and one in psychology of the abnormal. Wallon's study is an analysis of cases resulting from the war; and Abramson turns again to the old problem of measuring the functions of apprehension, imagination, memory and thought. Of the three papers in experimental psychology, that of Foucault reports the discovery of the intermittence of sensation in indirect vision, and of a critical region, of 5° or 6° in extent, surrounding the blind spot, where there are alterations of hue which resemble those in the eccentric retina. His stimuli were colored papers, and his experiments are too few in number to permit of a general conclusion. The study of Piéron is one of a series devoted to the general law that "when an organism submits to stimulation and reacts to it, the duration of the interval which elapses between stimulation and reaction varies inversely as the intensity of stimulus." This law has already been shown to hold for reflex actions in the lower animals and for reactions to sensation in man. The author now seeks a mathematical formulation for it, and finds that the decrease of reaction time regarded as a function of increase in intensity of stimulus follows an hyperbolic curve and tends asymptotically toward a limit. When checked by experiment the interpolation formula, while remaining true to type, is found to vary with sense modality. Piéron then undertakes an interpretative analysis of the formula by reference to the peripheral and central nervous system. The explana-

tion is cleverly worked out; but in so far as it relates to human reactions to sensation, it could be more readily accepted if an analysis were first made of the reaction consciousness as regards both the type of reaction and the nature of the 'sensation' to which reaction is made. In the third paper, Philippe reports that in the two-point limen experiment the nature of the cutaneous perception is conditioned upon the observer's knowledge of the stimulus. When, for example, the stimulus is quite unknown, a blindfolded observer neither perceives two discrete points nor is able to characterize the perception by reference to object. The same observer, however, as soon as he knows something about the stimulus, perceives patterns similar to those described in other investigations. Philippe thus challenges the traditional psychophysical experiment; and although he does not bring his results into relation with others in the same field, it is apparent that they support Titchener's critique of the two-point limen as an ethnological test, and Friedline's explanation of the failure of the experiment as a measure of fatigue.

Perhaps the most conclusive result of all the experimental papers is that of Rabaud's study of spiders. By careful observation of different species with different kinds of prey, and then by skilfully conducted experiments with artificial stimuli, he not only substantiates the fact already known that the spider reacts to a situation and not to an object (as, for example, to fly-on-web and not to fly as fly), but he is able to go further and to analyse the situation into a 'vibrating web.' By means of tuning forks held in contact with the web and vibrating with the proper frequency, intensity and intermittence he succeeded in inducing all the characteristic behavior of his spiders, including typical variations in the order of the acts employed in attacking and killing their prey.

The Notes and Reviews consist of two short papers, the one on a mode of economic brain-function (*Mode de fonctionnement économique du cerveau*) by A. Imbert, and the other on new laboratory apparatus (*Appareils nouveaux de laboratoire*) by H. Piéron. In the former it is argued that the cessation of respiration in muscular effort serves to relieve the brain from control of the necessary changes in rhythm of respiration, and thus permits a more economic employment of the brain's energy for the muscular effort. In the latter Piéron describes a pair of rotating discs whose absolute and relative rates of rotation may be controlled, a new differential photometer, and an aesthesiometer which is similar to von Frey's but in which a glass or quartz thread takes the place of the hair.

The *Chronique* contains the necrology of the two-year period, some personal notes, news of psychological periodicals, and reports of meetings, and of new psychological organizations, commissions, and institutes. The last of these topics has a summary account of the development of applied psychology in Germany.

As a whole this volume of the *Année* appeals to us as better than that of the preceding year; the original papers are more timely, and are in better accord with the present status of experimentation in their fields; while the digests are still of the high order which we have come to expect. The spirit of the book is that of physiological psychology; and there is, therefore, a certain fitness in the fact that this volume is dedicated to the memory of Henri Beaunis, who died in 1921, and who was the father of physiological psychology in France.

H. P. W.

Zeits. f. Psychologie. Bd. 91, Heft 1 u. 2. W. BLUMENFELD. 'Untersuchungen über die Formvisualität: I.' [*Formvisualität* is defined as the capacity to produce and manipulate in visual ideation bidimensional or tridimensional forms. The present paper offers a critical examination of some thirty tests or types of test, classified tentatively into four groups according as the material for ideation (bidimensional or tridimensional) is

given to perception or is itself ideational. There were thirty observers, principally mathematicians, engineers and architects. Provisional treatment of the results by rank-order methods shows that four of the examined tests possess a high symptomatic value; and their discussion enables the author to lay down with some definiteness the conditions which an adequate test of *Formvisualität* must meet.] E. R. JAENSCH. 'Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter: VI.' E. R. JAENSCH. 'Uebergang zu einer Schichtenanalyse des Bewusstseins und einiger seiner Substrate, gegründet auf die Strukturanalyse der eidetischen Entwicklungsschicht.' [Hitherto the study of eidetic phenomena has been confined to cross-sectioning. But if our adult worlds of perception and ideation have developed in time out of a matrix in which the *Anschaungsbild* predominated, then the study must be extended to longitudinal sections: we must trace the eidetic history of the individual from early childhood, and must also apply the eidetic touch-stone to the adult psychology of primitive peoples.] E. R. JAENSCH. 'VIII. Die Völkerkunde und der eidetische Tatsachenkreis.' [A study of Lévy-Bruhl's *Les fonctions mentales dans les sociétés inférieures* brings abundant evidence of the eidetic *Anlage* of primitives: extraordinary memory, confusion of *Anschauungsbild* with perception, phenomena of 'privileged' perception (shamans), mythological figures, multiplication of names (especially of spatially determined names) for concrete objects, phenomena of mystical participation (the 'internal' *Anschauungsbilder*), artistic productions.] Literaturbericht. F. HILLEBRAND. 'Berichtigung.'

Bd. 91, Heft 3-5. F. SCHUMANN. 'Untersuchungen über die psychologischen Grundprobleme der Tiefenwahrnehmung: III.' W. FUCHS. 'Experimentelle Untersuchungen über das simultane Hintereinandersehen auf derselben Sehrichtung.' [Reports experiments made by various methods to decide the question whether two objects in the field of vision can be seen simultaneously at different distances in the same visual direction, *i.e.*, whether monocularly one and the same retinal point or binocularly identical retinal points can mediate two impressions of different color at different distances; Helmholtz had affirmed, Hering denied the possibility. The result for stimuli in a single plane (*Flächengestalten*) is positive, under the condition that the two objects are apprehended as shaped wholes (*Ganzgestalten*), *i.e.*, that some parts of the nearer object project beyond the farther object,or better still that parts of each object project beyond the other, while the common field does not stand out as a special form. Transparency is abolished (1) if the form of the common field becomes dominant; the *O* then sees a 'mixed' color; (2) if attention is so occupied with points or contours of the farther object that the nearer figure ceases to be seen as shaped whole; and (3) if the nearer figure is so small as to be surrounded by the farther, and the farther figure itself is homogeneous. This last result may be reversed by voluntary 'inversion' (localisation of the nearer behind the farther figure), which can also turn opaque objects (strips of paper, iron rods) into transparent. Movement of one or both objects enhances (or may produce) the impression of transparency by enhancing (or producing) the impression of shaped wholes. The color of the projecting parts exerts a strong assimilatory influence on the color of the corresponding whole figure; the author promises to deal in a later paper with the general subject of color change as conditioned by apprehension of form.] W. BLUMENFELD. 'Untersuchungen über die Formvisualität, II.' [Tests with wire figures (tridimensional bending, ends not brought together; symmetrical pairs) lead to more satisfactory results than even the best of the tests previously discussed. The new tests are two: (1) comparison: two identical or symmetrical figures are shown in different positions; the subject is to decide whether they are identical or symmetrical; (2) recogni-

tion: a figure is exposed for impression (*Einprägung*); after its removal single figures, in different positions, are shown; the subject is to say whether or not the figure now shown is identical with the figure impressed. Different subjects work in different ways: by imaginative shifting of the seen figures, by imaginative adoption of a different standpoint toward them, by projecting the body into them, by 'traversing' them in imagination; in all these procedures, even if kinaesthesis plays a part, visual functions are essentially involved. The same thing appears to hold of identification by fingering.—A third test, in which the drawing of a figure was shown for perception or impression, and the subject was required to identify the figure in a group of simultaneously exposed identical or similar figures proved unsuccessful.] Literaturbericht.

Bd. 91, Heft 6. E. R. JAENSCH. 'Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter: VIII.' H. FREILING und E. R. JAENSCH. 'Der Aufbau der räumlichen Wahrnehmungen.' [Maintains that 'movement of attention' is the primary condition of spatial localisation. (1) Experiments on rotation show that *Anschauungsbild* and after-image are localised at the point of attention, uninfluenced by nystagmus. (2) Movement of attention has a dynamic component; *Anschauungsbilder* of heavy objects are moved less easily than those of light objects. (3) This component accounts for the absence of localisation in tones, and for the connection between space-perception and motility. (4) The antagonistic processes of attention (near-far, right-left), attested by the tendency to orthogonal localisation and by the pleasure in symmetry, derive (like those of color vision) from a self-regulative tendency of the organism. (5) A perceptive complex formed under the influence of movements of attention may persist for some time, engrammatically, after its conditions have ceased to be effective. The complex of the normal world of perception is a like engram of indefinitely stronger impression. (6) Movement of attention is prior to the space-values of the retina, but avails itself of organic structures and functions for the consolidation of perception.] E. R. JAENSCH. 'IX. Beziehungen von Erlebnisanalyse und Sprachwissenschaft, erläutert an den Verben der sinnlichen Wahrnehmung.' [Verbs of sense-perception take the accusative because the functions of perception, in the eidetic stage, stand in causal relation to the objects of perception,—influencing, changing, even constructing these objects.] E. F. HAZELHOFF und H. WIERSMA. 'Untersuchungen über die Frage der sensorischen Asymmetrie.' [Experiments with liminal faradisation, with passive application of light weights (for discrimination), and with the aesthesiometer show that, in right-handed persons, the left hand is the more sensitive. The authors suggest that the development of motor functions in the right hand has inhibited its sensitivity . In experiments with liminal faradisation, under sensory and motor instructions, the motor attitude is definitely more effective to reduce the right cases for the right than it is for the left hand. A single left-handed subject gave opposite results.] Literaturbericht. Einladung zum achten Kongress für experimentelle Psychologie in Leipzig.

Bd. 92, Heft 1 u. 2. E. R. JAENSCH. 'Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter: XII.' E. R. JAENSCH. 'Der Umbau der Wahrnehmungslehre und die Kantischen Weltanschauungen, nebst einer Erörterung über Johannes v. Kries' Grundlegung der Wahrnehmungslehre.' [The older sense-physiologists, Johannes Müller, Helmholtz, Aubert thought that they were following in the footsteps of Kant; they were, in fact, upon a side track. We find, nevertheless, if we look at current Kantian theories of knowledge, that they have a common psychological substrate, whose central problems are open to technically

psychological examination. It is the main thesis of the paper that such examination proves the psychology inadequate.—The author starts out from his own newer nativism, which stands to the older nativism as the Kantian a priori stands to the doctrine of innate ideas; it is concerned with the functions and methods whereby a plastic content is moulded. He then criticises von Kries' argument that the key to theory of perception is to be found in judgments of reflection; the eidetic psychology meets all difficulties, since it shows that the invariance of perceptive phenomena derives from that of the *Anschaungsbilder*. Next he disposes of the objections that genetic considerations are not decisive in the Kantian issue; that psychology has no relevance to the philosophical question; and that his own starting-point in psychological optics, *i.e.*, in phenomena of consciousness, confirms rather than refutes the Kantian position. Finally, he makes a plea for realistic as opposed to transcendental idealism: values, in particular, must not be banished from psychology.] T. SCHJELDERUP-EBBE. 'Weitere Beiträge zur Sozial- und Individualpsychologie des Haushuhns. [(1) Lists, describes, and refers to sex and occasion, all cries of the domestic fowl: laying note, cackle (or rhythmical note), crowing, warning-note, cooing, courting-note, cry, threat-note, nest-notes, lust-note, clucking, invitation-note, minor cry, alarm-note. In general, chicks do not learn their cries by imitation; with the single exception of the crowing of cockerels, the cries appear earlier in isolation than in the (inhibiting) company of older birds. (2) Hens blush under pain or emotion; the cock's face is too red and leathery for observation. Both sexes pay keen attention to face and head, less to body color, of their companions. (3) Recognition of locality is well developed; and hens brought up together may recognise each other after a three months' separation. (4) The law of pecking (Bd. 88) appears among cockerels earlier than among hen-chicks; it holds, in specialised form, for the companionship of young and old birds, and birds of different sex. A hen whose appearance is altered by sickness is attacked, probably because she is not recognised.] A. HERMANN-CZINER. 'Experimentelle Untersuchungen zum Problem des Verstehens.' [Reports experiments, with words (for the most part abstract conceptual terms) as stimuli, upon the process of reproductive understanding. The experience of understanding has three forms. (1) Simple understanding is characterised by isolation and unresolved unity; word and meaning are quasi-identical; there is no conscious apprehension of meaning, and no objective reference. (2) In represented understanding the representation of meaning may be impalpable (affective, motor) or palpable (mostly visual images); the experience is still unitary. (3) In explanatory understanding the meaning becomes conscious either in its determination by other meanings or in its relation to other meanings. The first of these experiences is still unitary, differing from simple understanding, not psychologically, but only by its intentional object; the second is discontinuous, implying the arousal either of fresh experiences of understanding or of thought-fixations and voluntary acts. The most frequent combinations are of (1) with (3) and of (2) with (3); conscious connection appears rarely in the former case, never in the latter; the first member is a closed unity. The three forms are, in certain circumstances, equally satisfactory. The visual images of (2) are, negatively, symbolic rather than representative; positively, they unify the whole experience and give the represented object a sort of form-quality. Form (3) occurs most frequently (owing to the nature of the *Aufgabe*), but may be characterised as irrelevant or unimportant (probably because the understanding is reproductive); it appears whenever the stimulus-word can be set in a thought-context. The results refute all sensualistic, associational and judgmental theories of understanding; they necessitate the assumption of experiences of the nature of form-qualities (for unitariness) and of unconscious processes (for discontinuity) and the adoption of a genetic point of view (for origin of representative images).] Literaturbericht.

Bd. 92, Heft 3 u. 4. H. HENNING. 'Starre eidetische Klang- und Schmerzbilder und die eidetische Konstellation.' [The eidetic experience is not so much an image as a state or attitude of mind in which the ego plays an active rôle. Pains, and impressions of the lower senses in general, tend to orient the ego toward reality, and so favor the eidetic attitude; supervening visual and auditory reproductions are then likely to assume eidetic form; it is thus, and not physiologically, that Urbantschitsch's results with heterogeneous stimulation are to be explained. We find, indeed, that persons who show no trace of the eidetic disposition in direct experiments may get eidetic experiences if several sense-departments are together brought into play; others get them only under the freer conditions of everyday life; others only in the hypnagogic state; others again only in dreams. The experiences are referable, physiologically, to centrifugal stimulation of the end-organ.—Various modes of auditory eidetic phenomena are described.] H. HENNING. 'Eine neuartige Komplexsynästhesie und Komplexzuordnung.' [In the case described, numbers and letters, presented visually or auditorily, are accompanied by a constant and compulsory synaesthetic experience, which is characterised in precise visual terms and yet is not a palpable visual image. The concomitant proves to be a complex quality, the total conscious attitude that is aroused by perception of a color, only with the color-perception itself left out. So we may experience the same Beethoven-complex-quality as we listen to a symphony or as we contemplate the Beethoven mask.] H. HENNING. 'Ein neuartiger Tiefeneindruck; die Versuchsanordnung des Rasterdiapositivs.' [Two stereoscopic positives, each of which has been taken through a fine square-meshed screen, are superposed on the back of a transparent celluloid plate, the front of which is etched in vertical lines of the same separation as the corresponding lines of the screen; in transmitted light the arrangement gives a plasticity far exceeding that of the stereoscope. A series of apparatus, ranging from this through the stereoscope, etc., to reversible perspective figures and thence finally to a plane figure, permits the demonstration of phenomenological tridimensionality. The impression of depth depends on detachment of background and apprehension of objective (*dinglich*) form. A study of Oriental pictures shows, in particular, the influence of ground-level on perspective interpretation.] T. CHIBA. 'Ueber die Asymmetrie der Unterschiedsempfindlichkeit.' [The times of discriminative reaction to pressure intensities, and the results of experiments on discrimination of tonal pitches and light intensities, prove or indicate (what is suggested by earlier studies) that sensible discrimination is better above the standard stimulus than below. This asymmetry is primarily a matter of relativity or contrast; in particular, attention in the ascending series goes to the variable, in the descending to the standard stimulus, so that ascending change is the more noticeable. There is also a tendency, in all sense-departments, for the sensation aroused by the standard stimulus to approach the proper or intrinsic sensation of its modality; hence decrease of stimulus becomes less noticeable than increase. Teleologically, the asymmetry of discrimination makes for protection of the sense-organ.] Literaturbericht.

Bd. 92, Heft 5 u. 6. W. FUCHS. 'Experimentelle Untersuchungen über die Aenderung von Farben unter dem Einfluss von Gestalten ("Angleichungserscheinungen").' [Reports experiments on color assimilation (1) when both forms are unitary surfaces (*e.g.*, episcotister set up before a large letter cut from colored paper); (2) when one form is composed of discrete elements (*e.g.*, row of colored circles, seen by reflection, the middlemost circle behind a square of a different color); and (3) when both forms are discrete (bicolored patterns of small colored discs, with or without reflection). The results are in all cases of the same type: we quote the summary given under (1). "The color of the projecting parts has a marked

influence upon the color of the common part; this, by assimilation, partakes as it were of the color of the projecting parts, and in its turn tries to assimilate the projecting parts in color to itself. The reciprocal assimilation is stronger, the more successful the pregnant apprehension of the total form of each surface. In the extreme case, each surface is of one and the same color in all its parts. The assimilation is a consequence of form-apprehension." Experiments on (4) negative after-images and (5) contrast bring out like facts; the transformations of after-image color with shift of form-apprehension are especially noteworthy. The results are offered in explanation of a large variety of phenomena: our overlooking of small objects that lie before us, the even color of a wall covered with monotone paper, the filling out of the blind spot, etc. An appendix discusses assimilation of form, size and localisation.] E. VOIGTLAENDER. 'Ueber die "Art" eines Menschen und das Erlebnis der "Maske;" eine psychologische Skizze.' [Manners, deportment, social carriage are very subtly compounded of genuine and deceptive elements, and stand in very various relation to the true inner character of a man; it is the task of characterology to work out this relation. If the position, character, rôle assigned to a man by his social environment does not accord with his true aims and nature, he is likely to assume a protective mask: Nietzsche is an outstanding instance.] K. MARBE. 'Ueber den Okkultismus; Erörterungen im Anschluss an v. Schrenck-Notzings Materialisationsphänomene.' [Schrenk-Notzing's book is valuable both as a cultural document and as an example of current popular psychology. Those of his kidney cannot be argued with; they have an unshakable will to believe. The writer, nevertheless, offers his laboratory as a testing place of any medium who is willing to be tested under strict conditions.] J. PLASSMANN. 'Psychologische Erfahrungen mit einem Zeitsignal, II.' [Cf. Bd. 88. The results show, as before, that the three-long signal is apprehended too early, the long-short somewhat too late, and the long-long-short considerably too late. Almost all the ten signals of the second minute (in which the beginning and not the end of the Morse stroke is apprehended) are perceived a little too late.] Literaturbericht. Zweiter Kongress für Aesthetik und Kunstwissenschaft.

Bd. 93, Heft 1 u. 2. G. E. MUELLER. 'Ueber Jaenschs Zurückführung des Simultankontrastes auf zentrale Transformation.' [Shows, by critical examination of Jaensch's procedures and by appeal to relevant facts of which Jaensch has failed to take account, that the reduction of contrast to transformation is impossible. Physiologically, the zone of transformation lies behind (centrad of) the zone of contrast.] W. KORTE. 'Ueber die Gestaltauffassung im indirekten Sehen.' [Campimetrical experiments (horizontal meridian) on the cognition of letters (large and small Roman, large and small German) and of meaningful and meaningless words (various lengths, small German letters). The cognition of a meaningful word begins with the total visual impression, which may of itself touch off a verbal report: this is almost invariably wrong. Then follows the identification of dominant letters (these may sometimes be small letters) as constituents of the total impression. Their identification arouses an auditory-motor image, *i.e.*, another total impression or *Gesamtform*, whose formation is essentially determined by meaningful connection. As soon as the dominant letters are noticed, a *Gestaltungsdrang* becomes effective: it operates together with visual form, auditory-motor form and meaningful connection to raise a word-meaning to a high state of preparedness; this word-meaning fuses, as a sort of complex-quality, with the other three moments The true from of the stimulus-word is then cognised. (The author thus takes a position between the extremes of Erdmann-Dodge and Zeitler, but nearer to Zeitler.) Among the conditions of mistaken reading we may note the abbreviation or condensation of the perceptive image in a certain region of the retina; the

author leaves its explanation open for special study.] A. GELB. 'Farbenpsychologische Untersuchungen, I.' A. GELB und R. GRANIT. 'Die Bedeutung von "Figur" und "Grund" für die Farbenschwelle: erste Mitteilung.' [Describes experiments in which a tiny disc of colored (usually red) light was thrown upon a colorless figure (Maltese cross at center of cardboard disc) or ground (rest of disc-surface). There were four discs in which the figure was darker than the ground; in all four the brightness of the figure, that of the ground, and the brightness-difference between figure and ground, were different; and there were four discs in which these brightness-relations were precisely reversed. The main result is that the color-limen is always higher for figure than for ground, and that the difference is greater, the more striking the difference between figure and ground. The explanation is that the little color-disc is itself a form, and that (owing to the tendency to pregnancy of form) the already formed figure resists the new form more energetically than the non-figured ground. Further experiments (incomplete) indicate that the *DL* of brightness also depends on the figure-ground difference.] Literaturbericht.

Bd. 93, Heft 3 bis 6. E. R. JAENSCH. 'Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter: XIII.' E. R. JAENSCH. 'Wahrnehmungslehre und Biologie: die Lehre von den Gesichtswahrnehmungen vom Standpunkt der Organologie des Auges (mit einer Beilage über José Ingenieros' Versuch einer Grundlegung der biologischen Psychologie, und seine Kritik der Psychologie in Deutschland).' [The structural relationship between the original visual phenomena and ideas is far closer than that between our developed visual perceptions and ideas. We should therefore expect to find that the eye is not a mere mechanism of reception and conduction, but rather a brain-organ. And we find, in fact, that the eye begins as a brain-pocket, while many features of its structure (inversion of the light-receptors, plexiform layers, centrifugal fibres) indicate a continuing brain-function; only at the fovea is the ordinary schema of a sense-organ realised. We find further that the incidence of a 'central factor' in perception over the lateral retina, in individuals of different age, and as between different individuals in general, is paralleled by anatomical characters. We may even suppose that in the foetus, as the eye is pure brain, so the visual experience is pure dream. The brain-nature of the visual apparatus accounts for the assimilation of our world of scientific ideas and theories to the world of visual perception.—The article contains, besides the critique of Ingenieros' book, a criticism of the *Gestalt*-psychology.] E. R. JAENSCH. 'XIV. Ausblicke auf kulturphilosophische und pädagogische Fragen und die Jugendbewegung unserer Zeit.' [Eidetic studies have emphasised the plasticity of human nature, and therefore justify a pedagogical optimism.] T. SCHJELDERUP-EBBE. 'Aufmerksamkeit bei Mücken und Fliegen'. [In the attitutde of attention *culex pipiens* raises its hind legs, *musca domestica* (with some other flies) plants its forelegs firmly and slightly raises the forepart of the body; the attitudes are teleological. Flies are very sensitive to temperature changes.] D. KATZ. 'Kleine Beiträge zur angewandten Tierpsychologie.' [Flies in movement are more easily caught than flies at rest; their visual perception during movement seems to be reduced. Under the influence of injurious stimuli both flies and gnats show a pronounced phototaxis.] D. KATZ und A. TOLL. 'Die Messung von Charakter- und Begabungsunterschieden bei Tieren (Versuche mit Hühnern).' [Five birds, which showed in human regard marked differences of character, were tested for memory, apprehension of relations, 'counting,' reaching a high-swung food-dish, leadership, reaching the food-dish by indirect paths, and spontaneous behavior (order of leaving and entering the coop). The rank orders are stable enough to suggest that it would be worth while to apply analogous tests to dogs.] T. SCHJELDERUP-

EBBE. 'Beiträge zur Analyse der Träume.' [In a well-defined half-conscious state between waking and sleeping 'sentences' form themselves, which show on analysis a marked flight of ideation, change of subject within the single sentence, an inexhaustible range of associations, and a manifold formation of meaningless words. The writer brings these sentences into relation with the speech and logic of dream and with some types of insanity.] T. G. HEGGE. 'Ueber Komplexbildung in verschiedenen Gebieten der Gedächtnistätigkeit.' [Compares in detail the formation of complexes in the purely visual learning of series, *e.g.*, of numbers or consonants with their formation in the learning of visually presented series of substantives. The tendencies in 'mechanical' and 'illustrative' memorising are essentially the same: stable connection, limitation, unification, differentiation; indeed, 'logical' grouping will be likely to intervene in mechanical learning, as there is always a mechanical component in logical learning. The differences are differences of means, not of ends: collective impression is of less importance for logical learning; unity is not so much a matter of form as of pragmatic and causal relations; range is a matter not of energy of attention and characteristic total form but rather of possibility of 'natural' connection; etc. In principle, the resemblances are far more striking than the differences.] Literaturbericht.

Bd. 94, Heft 1 u. 2. C. STUMPF. 'Singen und Sprechen.' [The main difference between speech and song, phenomenally regarded, is that speech makes wide and varied use of continuous tonal changes, whereas song (like music in general) confines itself in principle to fixed intervals and discrete tones. The difference is one of degree, and there are all sorts of approximations, mixtures, transitions; it is, nevertheless, primary and decisive. The emotive effects of speech, *e.g.*, are connected with continuous tonal changes, while those of music are connected with fixed intervals between fixed tones. Secondary differences consist in the larger part played in speech by noises, the reduced clearness of pitch in unaccented syllables, the greater freedom of temporal course.—Köhler thinks that the sounds of speech, as contrasted with those of music, lack the attribute of 'musical quality' (*c*-ness, *d*-ness); but the hypothesis does not bear examination. It leads, however, to a consideration of the phenomenology of continuous tonal change or tonal movement. The moving tone appears to differ only in degree from the resting tone; but, again, the difference is very great. Intensity is fairly clear; pitch less clear; clang-color still less clear; and musical quality least clear of all, distinguishable only by the trained ear in slow tonal movements. Köhler himself admits that a 'gliding' musical quality is conceivable.—Greater than the phenomenal differences between song and speech are those in the subjective attitude of singer and speaker; both general intention and special conscious set are markedly different.] E. R. JAENSCH. 'Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter: Schlusswort.' E. R. JAENSCH. 'Ueber Gegenwartsaufgaben der Jugendpsychologie.' [The eidetic studies so far published do not magnify a chance discovery; they are the outcome of a determinate plan, which goes back to 1909. The resulting child-psychology will replace, fruitfully, the economy-and-technique studies of experimental pedagogy.—The paper ends with a critique of the *Gestalt*-psychology.] A. PRANDTL. 'Die Koordination der Gehirn- und Bewusstseinsvorgänge.' [Coordination, in the sense of Mach and Avenarius, should replace the theories of parallelism and interactionism. Parallelism gives up the uniformity of the psychical life, and fails of explanation by way either of brain or of an unconscious; interactionism either gives up the uniformity of the physical or reduces the psychical to an epiphenomenon. Both theories have the same fundamental faults: they start out from the notion of 'action' (*Wirkung*), which is altogether unscientific (here the author interpolates a

discussion of the concept of causation), and they involve that doubling of perception and object of perception which, in its ultimate reach, was the object of Avenarius' polemic against introjection. What, then, remains? We are not helped by the distinction of act and content, subjective and objective, qualitative and quantitative: nothing remains but to fall back, with Mach and Avenarius, on a difference of our own standpoint over against the given. The objections that a 'physical' object must antedate its corresponding brain-process, and | that certain brain-conditioned experiences may still be 'physical,' are readily met.] Literaturbericht. Notiz. [Congress of Applied Psychology at Würzburg.]

Archives de psychologie. Tome xviii., No. 3. A. LEMAITRE. 'Les avatars d'une servante mystique: contribution à la psychologie religieuse.' [Describes the traits and experiences of a servant-girl of markedly mystical temperament: inner illumination rising to hallucination, abiding belief in the guidance and protection of a divine presence, admiration of nature, lyrism, altruism, clairvoyance. There are clear traces of the Electra complex. The mystical experiences have a biological value.] J. PIAGET et P. ROSSELLO. 'Note sur les types de description d'images chez l'enfant.' [Individual clinical tests of the description of pictures carried out with 43 children, 8 to 10 years of age, who a month earlier had been tested collectively in the class-room. There emerge four types: objective, subjective, intelligent, and superficial, which represent a general attitude of the subject and correspond with stable personal characters. The authors raise the question whether these types remain constant from test to test; and after evaluation of their own results, and comparison of them with the results of Binet and Lelesz, decide to leave it open; we need a great many more clinical examinations, and the publication of monographic 'case reports' like those usual in medicine.] H. FLOURNOY. 'Çiva androgyne: contribution à l'étude psychanalytique des principaux symboles et attributs d'une divinité hindoue.' [A psychoanalytic study of cult-objects, symbols and attributes of Shiva leads to the conclusion that the god is in reality androgynous. The only alternative to this mode of interpretation is recourse to simple 'imagination.' The worship of Shiva combines the glorification of sexual enjoyment with that of the principle of life.] Recueil des faits: documents et discussions. J. A. BIERENS DE HAAN. 'Sur les représentations des animaux.' [The results of Volkelt (1914) and other authors are insufficient to prove that the lower animals lack the perception of objects and can take account only of the total situation. Even the topographical memory of carrier-pigeons may be a character acquired during the evolution of the species and perhaps strengthened by practice.] G. VOLAIT. 'XVIIe Réunion des philosophes de la suisse romande: Rolle, 11 juin 1922.' Bibliographie. Nécrologie. [A. Lemaitre, W. Baade.] Notes diverses.

Tome xviii., No. 4. J. PIAGET. 'La pensée symbolique et la pensée de l'enfant.' [The thinking of children up to 7 or 8 years of age shows marked analogies, structural and functional, to the symbolic (prelogical, not paralogical) thinking of adults; it is characterised by lack of direction, amnesia, inability to frame definitions and to follow arguments, autism, confusion of self and not-self, self-centredness, preponderance of images (with condensation for generalisation and displacement for abstraction), overdeterminations and contradictions, failure to grasp causal relations or the relation of whole and part. Functionally, it is playful (Baldwin, Groos); structurally, it is simple, economical, restricted (Janet's 'weak tension'). The author leaves open all questions of genetic relationship.] S. SPIELREIN. 'Quelques analogies entre la pensée de l'enfant, celle de l'aphasique et la pensée subconsciente.' [The ideas of the child of 2½ years are associatively interconnected; both the ideas themselves and the associations are little

varied; ideas come to expression 'crumbled,' intermixed with bits of other ideas, and not clean-cut; thought in general is not singly directed and may be contradictory; it shows, accordingly, phenomena of perseveration and of dual determination ('crossing'); the train is kept going by emotive factors, interests. Analogies to the phenomena of perseveration, crossing and affective motivation may be found in subconscious thinking, in the language of children's drawings, and especially in the thought of the aphasic. All this evidence seems to point to the Jacksonian explanation of aphasia in terms of neural level rather than to the mere loss of verbal-motor images. It is possible, however, to bring the two theories into relation: subconscious thinking is kinaesthetic-visual, and the loss of connection between subconscious kinaesthetic images and conscious verbal thinking would mean reduction to a lower Jacksonian level.] G. Révész. 'Expériences sur la mémoire topographique et sur la découverte d'un système chez des enfants et des singes inférieurs.' [(1) A series of boxes, one containing chocolate, is laid before a child; when he has learned, in a number of trials, to go directly to the chocolate, the position of this box is changed. Beyond 4 years of age there is no evidence of associative inhibition; the second learning is easier than the first. Rhesus monkeys, who have passed the stage of mere mechanical memorising (fowls) but who do not understand the situation, are inadequate to the problem. (2) If the place of the chocolate is changed in the order 12345, children from 5 to 12 years of age master the system fairly easily; monkeys never grasp it, even if only two boxes are used. If the order is 15243, the problem is much more difficult, insoluble by 6-year-olds. The author analyses procedures in detail, and suggests that the two tests may (after a certain age) be employed to diagnose intelligence independently of age.] Recueil des faits: documents et discussions. E. Claparède. 'Formule commode pour le percentillage.' [If R is the rank corresponding with a given percentile, P this percentile, and n the number of individuals to be percentiled, then $R=1+[P\times(n-1)/100]$. This formula makes the percentiles 0 and 100 significant.] Bibliographie. Nécrologie. [E. Solari.] Notes diverses.

NOTES

Attribute and Sensation

In 1920-21 Dr. M. Yokoyama completed certain experimental series in a research that he had undertaken at my suggestion in the Psychological Laboratory of Clark University. At the end of that year Dr. Yokoyama returned to Japan, and since that time his duties at Keio University have prevented his bringing his results to publication. The general conclusion to be derived from his experiment was well understood before Dr. Yokoyama left the Clark Laboratory and was a matter of discussion among the laboratory group. Since the conclusions seem to me of particular interest, I believe that they should be reported without further delay in order that they may be available until Dr. Yokoyama's full account can appear. The quantitative data and protocols are not at hand, but I think there can be no doubt about the general findings. Dr. L. D. Boring and Dr. C. C. Pratt, who were with me *Os* in the experiment and were later acquainted with the results, have verified for me the statements of this Note. The responsibility for the Note is solely mine, though all credit for the experiment belongs to Dr. Yokoyama.

From the systematic position of psychologists like Wundt and Titchener the subject-matter of psychology is certain aspects of experience which stand up existentially under observation just as other aspects do in physics. Psychology and physics may be said to differ in their observational point of view. This distinction means that the fundamental materials of the two are identical though the systematic motives differ.[1]

In this setting there arises a problem of the nature of observational data: What is the simplest observational datum? Twenty years ago the answer to this question was tied up with the theory of mental elements. It was good psychology to say that the sensation was the observational ultimate and the attribute of the sensation the logical ultimate.[2]

This view of sensation, however, never quite fitted the facts. In experimental observations the *O* was always predetermined in favor of particular aspects of the sensory material. For a long time the fact of predetermination was taken more or less for granted, as something necessarily implicit in observation. In the Würzburg school, however, it became explicit in the *Aufgabe* and determining tendency. Since then the psychologist has to consider carefully instructions or other observational determinants very much as the physicist takes into account the constants of his instruments.

Such a change means that we cannot regard observation as the mere having of experience,[3] or psychological observation as more immediate than

[1] E. Mach, *Beiträge zur Analyse der Empfindungen*, 1885, *etc.*, and R. Avenarius, *Kritik der reinen Erfahrung*, 1888-90, are the primary sources of this view, which has been elaborated into psychological systems by O. Külpe, *Grundriss der Psychologie*, 1893, and by E. B. Titchener, *Text-Book of Psychology*, 1910 (*cf.* also this Journal, 1912, 23, 427-448, 485-508), and is implicit in other writers. Wundt's position was different ("mediate" *vs.* "immediate" experience), but not in so far as his view comes into relation with the content of this Note.

[2] *Cf.* E. B. Talbot, *Philos. Rev.*, 1895, 4, 154-166; M. W. Calkins, *Psychol. Rev.*, 1899, 6, 506-514; M. F. Washburn, *Philos. Rev.*, 1902, 11, 445-462; *Psychol. Rev.*, 1903, 10, 416-422.

[3] *Cf.* Mach's quotation of Krause: "Problem: To carry out the self-inspection of the Ego. Solution: It is carried out immediately." *Op. cit.*, trans. 1914, 20.

physical. Whatever is in mind under any particular conditions has always to be reconstructed by scientific logic from many observations. Under the best conditions we observe one attribute at a time, and then, because we can find any attribute when observation is changed and directed upon it, conclude that they were all "there". Because of this necessary limitation of scientific observation at any one moment, the entire sensation seldom constitutes the immediate observational datum. We observe only aspects of it, primary attributes or attributes of higher orders. These are the observational elements (Külpe's "conscious actuals"). The sensation may then be variously described, according to the interest of the writer. It is the "psychic real" (Külpe) if the writer's interest is in reality; in what is really "there" though it can be gotten but piecemeal by observation. It is a physiological construct (Rahn) if the interest is physiological; in the constant excitation in relation to which observation is determined. It is a systematic term (Titchener) if the emphasis is placed upon descriptive method; upon the scientist's reality induced from his observations.[4] And, if anyone is concerned still about elements, it is as fair to say that the sensation is the mental element as it is to say that the attribute is the observational element.

The question now arises as to the degree of limitation that observation places upon a sensory experience. With the *Aufgabe* as a new tool we may very properly seek to measure its effect. In concrete form this question becomes: How much does the predetermination to observe one attribute of a sensory experience prevent the observation of other attributes? The extreme case is especially interesting. If an *O* is maximally determined for one attribute, can he observe any other attributes at all? This question gives the setting for Yokoyama's problem.

Historically, on the experimental side, the conditions of the psychophysical experiment give the best assurance for intensive observational determination. Judgments near the limen are difficult to make and spur the *O* to the most concentrated observation. The continuous repetition of presentation and judgment habituates the *O* in the single type of observation. The presentations should, moreover, be brief, partly because they thus render the judgments more difficult and so help to fix observation, and partly because they reduce the likelihood of change in the experience which makes observation equivocal, more complicated, and hence more difficult of fixation. For these reasons Yokoyama had his *O*s pass relative judgments upon two stimuli for the determination of a differential limen, and limited his presentations to considerably less than 100σ.

He used a dark exposure-box, similar to the Dodge tachistoscope, in which the *O*, fixating a tiny spot of light, was presented, after warning signals, with two rectangles of light, end to end. The rectangles could be varied relatively in brightness and in length.

In one series the *O* was predetermined to observe relative intensity. He was to pay attention to intensity only and to judge the one stimulus with respect to the other. The instruction was vigorous, the time was short, the sensory difference was small, and the task was continued for many observations. Altogether the *O* may be supposed to have been as definitely predisposed for intensity as he could have been. At irregular intervals in this series, however, and not oftener than once in fifty trials nor more than twice in a session, the *E* broke in, immediately after the exposure and often before the judgment of intensity could be voiced, with a secondary instruction: "How about extent?" The *O* was then expected to give *ex post facto* a judgment of relative extents.

In other series the primary determination was for extent, and intensity was called for at infrequent intervals.

[4] C. Rahn, *Psychol. Monog.*, 1913, No. 67: Titchener, this JOURNAL, 1915, 26, 258-267.

After a judgment the *O* wrote an introspective account of the consciousness.

We may in this experiment speak of the instruction for an attributive judgment given before the stimulus in all the trials as the 'primary determinant,' and the instruction occasionally interjected *ex post facto* after the stimulus as the 'secondary determininant.' In these terms the principal results are as follows.

(*a*) Under the primary determinant the judgment follows immediately, often without the mediation of any content other than the perception of the stimulus. This is the familiar consciousness which the psychophysical experiment often brings out.

(*b*) The judgment under the secondary determinant is usually delayed. The determinant, moreover, never acts retroactively directly upon the past perception to give an immediate judgment of the perception (except in the sense that the judgment refers logically to the perception, and that, being mediated by a surrogate which is in turn conditioned by the perception, it has an indirect functional relation to the perception). A surrogate of the perception is always present after the introduction of the secondary determinant. Sometimes the original perception is still persisting in visual imaginal terms of the order of the memory after-image when the secondary determinant is given. In such cases the judgment appears to the *O* to come quickly. When the experience has lapsed, however, it has to be reinstated as an imaginal surrogate (also usually visual for these *O*s), and then the judgment issues. These cases account for the delay. Usually an *O* accepts the surrogate as adequate; only occasionally does he become aware of a dilemma, and doubt his judgment because it is not based directly upon the process judged, or fail to make a judgment because it is impossible to 'decide' to make a judgment after the experiential process is past. I am not able to say without the protocols in hand whether there is any difference between the surrogate and the original perception.

(*c*) It is plain that the original perception under the primary determinant is inadequate to a judgment of any difference other than the attribute upon which the primary determinant is directed. If the capacity to make such a relative judgment is taken as the test of the presence or absence of an attribute, then it would be necessary to conclude that under a strong determinant one attribute might be present and the others missing. Thus the complete selectivity of a determination would be established. But the introspections of these *O*s show that, although they cannot make relative judgments upon the attributes for which they are not predetermined, they nevertheless can appeal to various aspects in the description of the perception. For instance, when determined for intensity, they make the judgment of intensity immediately and have a good deal to say about intensity in their descriptions of the perception; but they also frequently note something about extent,— 'lengths', 'widths,' and even the 'square shape' of the corners of the rectangles. Similarly when determined for extent, they note 'lengths' as the primary datum, but frequently mention the intensive pattern of the field. In a few protocols *O*s even insisted on the original existential presence of an attribute upon which they made no judgment or upon which they could not make a judgment without the intermediation of a surrogate.

From these results several conclusions can be derived.

(1) An instructional determination is always predetermination. A determinant cannot act retroactively.

(2) The apparent readiness with which a determinant seems to act *ex post facto* upon a perception is due to the intermediation of a subsequent surrogate for the perception. The surrogate may be a reinstatement or a persistence of the perception. It is not necessarily discrete from the perception, however; and in the case of persistence there is not even a tem-

poral gap. The adequacy of the surrogate under these relatively refined conditions of introspective observation argues that, for purposes of observation, the surrogate may be, and ordinarily is, identified with the perception. The total perception which underlies the judgmental report is a changing process, but the fact that it functions for observation as if stable means that a perception for the purposes of observation is a temporal 'mental object,' that is to say, a temporal series of events is integrated into an 'objective' whole in the sense that it functions for report as if it were a fixed datum.

(3) Moreover, it is interesting to note that Yokoyama did not succeed in reducing a perception observationally to a single attribute. To fail under given observational conditions in the reduction of the perception to a single term is not to prove that it cannot be done, and to find more than one attribute ready for descriptive report is not to prove that all the attributes were observationally available. We cannot conclude from these experiments anything more than that predetermination limits the observational range and that it is nevertheless very difficult to reduce it to complete simplicity.

(4) Finally, we may note that the test of the existential presence or absence of an attributive dimension does not lie in the capacity of the *O* to make relative judgments upon the attribute. Description is the finer instrument, and even it is subject to difficulties which arise out of the indeterminate nature of the 'mental object.'

As is so often the case when the experiment is over, it is easy to see that the conditions were not ideal. The determination was not entirely univocal; it was simply more nearly univocal than the determination has been in most psychophysical work upon which the theory of attributes has been based. One tends under these conditions to get space rather than pure extensity, and there is no agreement among psychologists as to the nature of intensity as a visual attribute. There were good reasons for taking two stimuli and making the judgments relative, but the experience was thereby complicated. It is not plain how these difficulties can be overcome. It seemed easiest to Yokoyama to use vision, and he could not use its quality as a variable with such short exposures on account of the *Anklingen*-times of the colors.

It is also natural to object that the occasional interjection of the secondary determinant spoiled the univocality of the primary determinant. I do not know how to combat this objection, but I believe it to be invalid. The evidence is the *Os*' reports. Moreover they were experienced *Os* (L. D. Boring, M. B. Pratt, C. C. Pratt, and myself); two other, less experienced *Os* were discarded on the basis of their reports. The person who makes this objection should first try making hundreds of difficult observations with a secondary instruction interjected not oftener than once in fifty times or twice in an hour, and with the knowledge, not only that he does not have to keep the secondary instruction in mind, but further that he must try his best not to keep it in mind. This person will find, I think, that the secondary instruction lapses entirely and that its interjection comes always as an unexpected shock.

E. G. B.

The Overlooking of Familiar Objects

In his recent study of the phenomena of assimilation, *i.e.*, of changes of color under the influence of form (*Zts. f. Psych.*, 92, 1923, 249 ff.), W. Fuchs suggests that our overlooking of things in everyday life, even when we may be gazing steadily at them, is due to the levelling out of their color, by as-

similation, to the color of their surroundings (307 f.). The explanation may hold in certain instances; it certainly does not hold in those with which I am most familiar.

I very often 'lose' things in the tangle on my desk. I can always find, without trouble, objects that look more or less alike from whatever side they are regarded (*e.g.*, round or oval boxes) and objects that 'naturally' fall into all sorts of positions (pencils, pens, letter-opener, paper-cutter); any the least exposed portion of these objects touches off cognition. I have great difficulty, on the other hand, with spectacles and desk-shears. I habitually lay these things down, after use, in a particular position; if, in the shuffle of papers and desk-furniture, they are brought into other positions, I do not see them; sometimes, though I know that the object is lying before me, I am obliged to ask the assistance of another pair of eyes.

My habit of laying down the glasses in a particular way began with a voluntary act; that of laying down the shears, so far as I can recall, grew up of itself under the conditions of usage. I have now, and have had for a good many years, definite schematic visual images of these two things in their 'proper' positions. I have also a definite motor set or attitude for picking up and replacing the glasses (both hands) and for taking up the shears (right hand). Over and over again, when I have been looking for the lost article, I have caught myself looking for it in terms of the visual image; I do not project the image on the desk, but rather carry it 'in my eyes;' and what I try to do is to fit the image over something that I see in the litter before me. When the image is clear, I do not notice the set (at least I have no record to that effect), though undoubtedly the set is there. When the image fails to appear (it may be kept under, *e.g.*, by other imagery due to visual preoccupation), I am able by shift to the observational attitude to remark the set. If, then, the image does not fit any visible object, or if there is nothing in the visual field that discharges the motor set into overt movement, then I fail to see what I am looking for.

Discovery may come about in various ways. Ordinarily, of course, I want the glasses or shears at the moment when I look for them, so that, after a discursive view of the desk-surface, I begin to move things about. Oftentimes, however, I have paused to observe image or attitude, and then have dismissed the image or relaxed the attitude, with the result that the object stares me recognisably in the face. Sometimes, when I am deeply absorbed, I sit helplessly, looking and looking without result; then I either give up the search, for the time being, or call in assistance.

There is, evidently, no need to invoke phenomena of color assimilation to explain these oversights; visual image and motor set are enough. I can, however, offer positive testimony against assimilation. It frequently happens, when the object is finally discovered in the 'wrong' position, that I have a flash of amused or irritated recognition, as if I should say "Oh, *you*, is it? I saw *you* before." I have never noticed, in this recognitive experience, any hint of color change. Moreover, when one considers the multiplicity of small, variously colored objects with which the desk is covered, it is difficult to see how assimilation could there come into play.

E. B. T.

Dr. Johnson on Reaction-Time Experiments

Dr. H. M. Johnson, in his summary of the studies of the past decade that have involved measurements of reaction-time[1], reviews two of my Studies with E. E. Cassel[2], and offers such criticisms of one of them—the

[1] *Psych. Bull.*, 20, 1923, 562-589.

[2] E. E. Cassel and K. M. Dallenbach, The Effect of Auditory Distraction upon the Sensory Reaction, this Journal, 29, 1918, 129-143; An Objective Measure of Attributive Clearness, *ibid.*, 204-207.

Study on "An Objective Measure of Clearness"—that a reply seems necessary. Dr. Johnson challenges our introspective findings, questions our data, charges us with a misapplication of method, and instructs us in the method of computing a correlation.

(1) The "introspective findings" which Dr. Johnson calls into question and the excerpt which he quotes as illustrative of them are, however, drawn, neither from the body of the experiment nor from the conclusions, but from a footnote[3] at the end of the article. Moreover, his omissions from the passage quoted are such that its implication is seriously altered.

(2) Dr. Johnson phrases his objection as follows: "The introspective findings might, of course, be challenged. Reaction certainly tends to become quicker with practice; and as in other instances of habit-formation, some reactors, including myself, have incidentally noted a tendency for the degree of consciousness of everything connected with the response to diminish, and almost to vanish, with continued practice."[4] Has he overlooked the fact that we were working not with a muscular but with a sensory reaction; or does he, as a behaviorist, deny the distinction? If the former, this correction meets his difficulty; if the latter, he takes his position in the face of experimental evidence.

(3) Dr. Johnson claims, further, that our "data do not support our reasoning;" wherein they fail he does not say. Except for this dogmatic statement, he does not again touch upon our results or upon our problem, but turns to an exposition of a method for obtaining, from our data, the correlation between the individual measurements of reaction and attributive clearness.[5] With this exposition we are in substantial agreement; but we regard it as irrelevant.

Our experiment fell into two parts. In the first part *O*, who was trained in the introspection of attributive clearness, gave, after every series of 10 reactions, an estimation, in terms of a scale of clearness-values which had been worked out in earlier studies,[6] of the clearness of the mental processes experienced during that period. The results showed that the average time of the simple sensory reactions and the degree of precision as expressed by the m.v. are both, under our conditions, inversely correlated with the introspectively by reported clearness-values. These results, however, were open (as we pointed out[7]) to the obvious objection that *O*'s reports, inasmuch as they had been delayed until after the completion of 10 reactions, were rather general impressions than true introspective descriptions, and that the figures, inasmuch as they were averages of averages, were artificial and not truly representative. *O* was therefore required during the last 700 experiments to report, immediately after every reaction, the clearness of the mental processes during the reaction. In the table in which these data are presented[8] we give (1) the different clearness values reported, (2) the frequency of their report, (3) the average reaction times for the different clearnesses, and (4) the mean variations of these averages. We computed the correlations, as we explicitly said, between clearness and average reaction time, *i.e.*, between (1) and (3), and between clearness and mean variation, *i.e.*, between (1) and (4). But Dr. Johnson objects, "Such treatment disregards the variations of time corresponding to each degree of clear-

[3] *Op. cit.*, 207, footnote 12.

[4] *Op. cit.*, 578.

[5] The attempt to measure attention in terms of the length of the sensory reaction was early proposed and early given up; *cf.* L. R. Geissler, this JOURNAL, 20, 1909, 496 ff. The coefficient that Dr. Johnson obtained from our data was small (-.25), and the negative evidence is thus further corroborated.

[6] K. M. Dallenbach, this JOURNAL, 24, 1913, 468; 27, 1916, 445.

[7] *Op. cit.*, 206. [8] *Op. cit.*, Table II, 206.

ness."[9] Surely: and if he had not been prepossessed by his own argument, he would have seen that it was just such variations that we wished to disregard,— or rather, to eliminate by averaging.

The length of the simple sensory reaction is determined by many conditions beside attributive clearness. Some of these, such as practice, habituation, and fatigue, were eliminated (so far as elimination is experimentally possible) by the conditions under which our work was done; others, such as attitude and attributive clearness, were introspectively noted and thus controlled; and still others, unknown and adventitious (Dr. Johnson will hardly maintain that the conditions named are the only ones that affect the reaction time), were beyond our control.

We based our procedure of averaging upon the assumption that the length of the reaction was a function of the chance distribution of attitude, clearness, and the unknown conditions. Of these conditions clearness alone was measured. We thought, by averaging the reaction times that were paired with the various degrees of clearness, to eliminate the effect of the other conditions and thus to discover the relations that existed between clearness and reaction. Since the correlation thus obtained was very high and the PEs were very low, since the results of the two parts of the experiment agreed, and since the relation persisted through marked shifts of attitude, we felt justified in concluding that under our conditions (we explicitly forbore generalization) "attributive clearness could be measured by the *average* duration and the *mean variation* of the simple sensory reaction."[10] Our procedure may be questioned; but we can not well be criticised for not "treating each paired measurement as a single datum";[11] we knew beforehand that that procedure would be futile.

(4) Dr. Johnson's review is not always accurate, either in quotation or in report. We have already mentioned a quotation the sense of which has been changed by omission. Again, in the last paragraph of his review[12], he draws a quotation from its context in the first part and applies it to the data of the second part. In yet another instance,[13] he credits us with a conclusion which nowhere appears in our paper.

Inaccuracy of report is shown by the following: "Two flashes of a Geissler tube serving as preparatory signals were given in succession 3″ and 1.8″ respectively, before the stimulus."[14] We used, not a Geissler tube, but a "2.9 volt electric globe" hooded "by four layers of black cloth." The preparatory signals occurred "3 sec. and . . . . . 1½ sec. before the occurrence of the stimulus." Dr. Johnson is also in error in his report of the occurrence and distribution of the control experiments among the distraction experiments.

K. M. D.

Core and Context in the Drowsy State

The experience of which I write occurred one morning in the early Spring of 1921. It had been my custom to go to bed about 11 o'clock at night, and to be waked in the morning by the light of the rising sun which flooded my room and by reflection fell upon my closed eyelids. My waking was always in concomitant variation with the rise of the sun. On winter mornings I would wake about 7 o'clock, at which time the first sunlight penetrated my eyelids. As the sun rose earlier, I woke earlier, until at the end of February I was waking between 6 and 6.30.

One morning early in March, between 3 and 4 o'clock as I later observed, I was still in the drowsy state when the following vivid dream occurred to me. 'I was awake and in bed. The room was dark, save for a huge tongue of flames which would intermittently leap from the open door of a large,

[9] *Op. cit.*, 570. [10] *Op. cit.*, 207. Italics are mine. [11] *Op. cit.*, 579.
[12] *Op. cit.*, 580. [13] *Op. cit.*, 578. [14] *Op. cit.*, 577.

red-hot, pot-bellied stove in the center of the floor and illumine the room. Fear was aroused in me less the flames might catch the bed-clothing'.

The fear mentioned at the end of the dream was the direct cause of my sudden waking to discover that the first electric storm of the spring was threatening. There was no thunder, but merely the intermittent flashes of lightning far off. The sky was dark. I observed the time to be 3.30.

It became at once apparent to me what, in a psychological sense, had occurred to me. The intermittent visual stimulation of my retina through my closed eyelids by the flashes of lightning had been necessarily vague, in fact, too vague to arouse centrally the entirely appropriate context. In place of such a context, this vague core of pure sense-perception aroused in my drowsy consciousness the context given above as my dream, in which the flames intermittently belched forth from a red-hot, pot-bellied stove. In reality there was no such stove in my bed-room.

The striking features of the context are as follows. (1) The context in my drowsy state was composed of two elements: ideas and feeling. (2) The ideas were visual concrete. (3) The feeling-element was complex in character, *i.e.*, it was the emotion of fear. The significance of fear in the context becomes intelligible either in the sense of my instinctive reaction to sudden stimuli or in the sense of my life-long emotional reaction to lightning as well as to fire. (4) Although the context was not entirely appropriate to the actual core of pure sense-perception, it clearly represented the attempt of the drowsy mind to arrive at the appropriate one.

West Virginia Collegiate Institute F. C. SUMNER

A BLOWER FOR THE GALTON WHISTLE

In an experiment conducted in the Harvard Psychological Laboratory it is necessary to produce a continuous Galton whistle tone for several minutes. To meet this requirement a blower was devised which blows the whistle for about 25 min. at a constant air pressure of 100 mm. of water.

The blower is made of three five-gallon glass bottles, such as are used in the sale of spring water. The lowest bottle, hereinafter called No. 1, stands on the floor; the next, No. 2, is on a low table (21 in. high); No. 3 is on a table of ordinary height (32 in.). No. 2 is filled with water, which is siphoned into No. 1 through tubes (glass with some rubber sections). The incoming water forces air from No. 1 to the whistle. The glass tube leading into No. 1 should not extend to the bottom, as the rise of the water in the bottle on the floor would decrease the hydraulic head, which determines the pressure, if the head were to be measured to a changing level instead of to a fixed position at the neck of the bottle; hence the inlet should deliver the water only to the neck of the bottle, and, to insure silence, should divert the water to the side of the bottle instead of letting it drop straight down. The level of the water in No. 2 must not be allowed to fall, since any change in it also changes the effective head. To keep it constant, water is siphoned into No. 2 from No. 3. The level in No. 2 should be marked at the beginning of an experiment and kept as near as possible to that mark if constant pressure is desired. The tube from No. 3 to No. 2 should be somewhat larger than that from No. 2 to No. 1, and one of the rubber connectors should be fitted with a clamp, which is adjusted until the right amount of water is flowing into the middle bottle to keep the level at the mark. If the whistle is blown as long as 15 or 20 min. and one wishes to have the air pressure so nearly constant that no change in the height of the water in a manometer is perceptible, it may be necessary to adjust this clamp from No. 3 to No. 2 once or twice during that time. A little deviation, say, half an inch either above or below the mark on No. 2, does not produce any perceptible change in the height of the water column of the manometer. When No. 1 is full, No. 3 is empty; to blow the whistle for

another period it is necessary to interchange these two bottles. The bottles are, of course, equipped where necessary with rubber stoppers with holes to admit glass tubes, and at appropriate places there are sections of rubber tubing with clamps to regulate the flow of water or of air.

The advantages of this blower are its cheapness, silence, simplicity of construction, and the absolute constancy of the air pressure.

Harvard University FRANK A. PATTIE, JR.

A NEW PSYCHOLOGICAL SPECTRUM

The Cornell demonstrational laboratory possesses a psychological spectrum,[1] which Mr. L. A. Fuertes painted for Professor Titchener in 1903. The spectrum is painted in oils upon a canvas strip of approximately 4⅛ by 72⅛ in.; the strip is set in a black frame 14 by 84 in.; and it is faced with plate glass, in front of which are four flaps of black cardboard, hinged below, which can be turned down to show any or all of the four qualitative series. This spectrum has served its purpose very well; the one serious objection to it is that the four qualitative series, copied too closely from the Prang chart of the physical spectrum, are of uneven length.

We have lately been trying out, for demonstrational purposes, the colours of wall calcimine. These colours have a hard brilliance which, as compared with the soft depth of oil colours, seems almost luminous. A very effective psychological spectrum may be made as follows.

The spectrum is 5 in. high and 80 in. long; each one of the four series R-Y, Y-G, G-B, B-R is 20 in. in length. The colours are mixed by weight to give 40 units to a series, so that every unit covers half an inch of the full 20 in. laid off for the series. The 40 sensory steps, even where they are most evident, are only slightly above the limen.

For every series—we take the R-Y series as illustrative of all—three pairs of values must be found: (1) the amount of Y which, added to a known amount of R, will furnish a hue just perceptibly yellower than pure R; (2) the amount of R which, added to a known amount of Y, will furnish a hue just redder than pure Y; and (3) the proportion of R to Y which will give the sensory middle term between pure R and pure Y, *i.e.*, a hue which looks as much like R as like Y. These data, when the relative amounts have been converted into suitable absolute amounts, give three points on a curve of diminishing amounts of R and on a curve of increasing amounts of Y, as one passes from pure R to pure Y. More points may, of course, be determined, if one is not sure of the bisection of the qualitative line; we have found, however, that these three are enough. When the curves are plotted, the units of ordinates and abscissae should be chosen of such relative magnitudes that interpolation will be easy, since it is from these curves that the amounts of the colours are to be read. In our instance the axis of abscissae represents the sensory length of the R-Y series. If it is divided into steps just less than the DL, there will be many members to mix, and the mixing takes a great deal of time. If it is divided into steps just larger than the DL, there are fewer members to mix, but the spreading of the pigment and its blending under the brush are not easy, at any rate for an unskilled workman. Whichever plan one adopts, the chosen steps are laid off on the axis of abscissae, and ordinates are erected upon them. The curves cut the ordinates at the points representing the absolute amounts of R and Y needed for the corresponding sensory member. It is worth while to scrutinise the amounts read from the curves, and to make sure that they are sufficiently accurate, before mixtures are made according to them; thus, the values as read might be 35, 33, 32, 30, 29, etc., which should be corrected by inspection to read 35, 33, 31, 30, 29, etc.

[1] E. B. Titchener, Class Experiments and Demonstration Apparatus, this JOURNAL, 14, 1903, 178.

In the spectrum which we have already made the sensory borders of the colour units are not eliminated. In our lecture room, students even in the front row are far enough away to see the spectrum, thus prepared, as a smooth line of hues. We are now making another spectrum with the border lines eliminated, so that there shall be a uniform transition along the colour series even under the conditions of near observation. The blending of unit with unit is not easy to execute; but it is possible to spray the surface of the calcimine first laid, and so to keep it in the form of a paste which may be blended by the brush for the removal of the boundary lines.

Cornell University H. G. BISHOP

THE ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The thirty-second annual meeting of the American Psychological Association was held at the University of Wisconsin, Dec. 27th to 29th 1923. There were about one hundred and twenty-five present at the banquet, which figures give an idea of the size of the attendance at the various sessions. Naturally the proportion of members from the Middle West was greater than when the Association meets on the Atlantic Coast; there were, however, a considerable number of members from the eastern colleges as well as from the Far West. As abstracts of the papers have already been published, it does not seem necessary to give a summary of the addresses in this place. In making this report I desire merely to point out some of the features which differentiate this meeting from the previous ones.

The programme was very well arranged in that the parallel sessions only partially overlapped. This plan enabled one, as a rule, to hear all the papers one was particularly interested in. Further, there were as a rule only four or five papers in each session, so that there was plenty of time for discussion, of which there seemed to me to be more than the usual amount. I think this was one of the most encouraging features of the meeting. Many of us have felt for some time that unless some means were found for stimulating more animated discussions, the formal side of the meeting could not be considered very successful.

There were a large number of papers on General Psychology, so that two sessions were devoted to this subject. Behaviorism still seems to arouse the greatest interest, if one can judge from the liveliness of the discussions. The *Gestalt*-theory made its first appearance on the programme as a tendency in psychological thinking. Without doubt, it will appear more frequently in the papers of subsequent meetings.

The experimental papers also occupied two sessions. The reports were very encouraging to laboratory psychologists, who hope that these sessions will grow in number and importance. There were, on the other hand, fewer papers than usual on mental measurement.

The three outstanding innovations in the programme were a session for Industrial Psychology, a session for papers presented by graduate students, and an informal conference of experimentalists. The industrial group, which was presided over by Professor Bingham, was chiefly concerned with vocational guidance and selection, and the study of personality. The session was considered very successful by those present and it was voted to arrange for a similar session next year.

According to the opinion of some of the members, who were present at the session for graduate students, this new plan of allowing the younger men to take an active part in the meeting proved to be an extremely good one. It was much more than a market for young instructors. The papers, for the most part, were very interesting and aroused animated and somewhat informal discussions. No doubt, the men gained valuable experience in defending their views, and the older men were able to obtain some idea of the general tendency among the younger generation of psychologists.

At the informal meeting of experimentalists about thirty-five members were present. Professor Raymond Dodge presided and almost all the large laboratories were represented. The general discussion was on experimental problems of *Gestalt*. There were also several brief reports from the various laboratories. The members considered the meeting very well worth while and voted to continue it next year. Professor Boring was elected chairman of the next meeting.

The symposium on the Contribution of Freudianism to Psychology did not seem to me to be very successful. I do not mean to reflect upon the addresses of the participants, but upon the general nature of symposia as they are at present conducted. I have sat through a number of them during the last year, and, so far as I could perceive, they might as well have been termed a special session devoted to papers on a particular topic. The main difference seems to me to be that the symposia are more general in nature and for that very reason, as a rule, less valuable than special sessions. For a symposium to have a right on the programme there should be a central theme which is supported against attacks from opponents. Such an arrangement requires considerable time and preparation; and it is to be hoped that when a symposium is again planned selections will be made and a pivotal paper commenced at least eight months before the meeting.

Professor L. M. Terman, in his presidential address, defended research in the field of mental testing as a legitimate part of experimental psychology. His paper was a pleasing combination of serious discussion and delightful witticism. In his clever defence of his position he showed much tact and dignity, and the audience must have been impressed by his sincerity and deep-rooted faith in the value of the work in which he is engaged.

Harvard University H. S. LANGFELD

TORONTO MEETING OF THE BRITISH ASSOCIATION

A meeting of the British Association for the Advancement of Science will be held at Toronto, from August 6th to 13th, 1924.

The Association has recently added to its various Sections a Section of Psychology. The Organising Committee of the Psychological Section believes that the meeting will afford an unique opportunity for intercourse between psychologists of Canada, the United States, and Great Britain; and accordingly issues an invitation to any psychologist in the United States or Canada, who may desire to do so, to join the Association as a member for the Meeting, and to contribute a paper or attend the discussions.

Professor McDougall, F.R.S., of Harvard University, has accepted appointment as President of the Section; and it is hoped that he will deliver the presidential address. Other papers have been promised by Dr. Morton Prince, Dr. Brett, Dr. Bott, Dr. Bridges, Dr. Sandiford, Dr. Tait, Dr. Myers, Dr. Burt, Dr. Drever, Mr. Flügel, Dr. Miles, Professor Pear, and other leading American and British psychologists.

Joint Meetings have been arranged with the following Sections:

Section H (Anthropology), to discuss "Racial Mental Differences."

Section I (Physiology), to discuss "Application of Physiology and Psychology to Industrial Efficiency."

Section L (Education), to discuss "Tests for Scholarships and Promotion."

Papers should not require more than 20-30 minutes to read; and intending contributors should send a notice of their papers before the end of March to the Recorder of the Psychological Section (British Association), The Psychological Laboratory, The University, Leeds, England. Further

particulars as to membership for the meeting can be had from the Secretary, British Association, Burlington House, Piccadilly, London, W.1, England; and notices of the general arrangements will be issued in due course by him and by the Hon. Local Secretary in Toronto.

LL. WYNN JONES, Recorder
CYRIL BURT, President (1923)
W. MCDOUGALL, President (1924)

PUBLICATION IN GERMANY

Shortly after sending to the JOURNAL the Note concerning the serious curtailment of German publication, I received information from various men in Germany to the effect that the situation is not as serious as Professor Stern's words seemed to indicate. The representative of a Leipzig bookseller gave it as his 'unofficial' opinion that there is at present about a twenty percent. curtailment in the publication of books and periodicals.

Harvard University H. S. LANGFELD

INTERNATIONAL CONGRESS OF PHILOSOPHY

The fifth International Congress of Philosophy will be held in Naples, on the occasion of the seventh centenary of the foundation of the Royal University of Naples, May 5-9, 1924. Two anniversaries will be commemorated: the six hundred and fiftieth anniversary of the death of St. Thomas Aquinas (with Cardinal Mercier as official speaker) and the two hundredth anniversary of the birth of Kant (Professor A. Liebert). The Congress will be divided into ten sections: (1) metaphysics, logic, gnoseology; (2) aesthetics; (3) ethics; (4) history and philosophy of religion; (5) philosophy of law; (6) history and philosophy of science; (7) psychology; (8) pedagogy; (9) sociology; and (10) history of philosophy. The general secretary is Professor Guido della Valle, 259 Piazza Salvator Rosa, Naples 35.

ARBEITEN ZUR ENTWICKLUNGSPSYCHOLOGIE

Professor Krueger's series of *Arbeiten* seems to be firmly established as the successor of Wundt's *Psychologische Studien*. The numbers so far published are as follows:

(1) F. Krueger, Ueber Entwicklungspsychologie, ihre sachliche und geschichtliche Notwendigkeit, pp. x + 232.

(2) H. Volkelt, Ueber die Vorstellungen der Tiere, pp. iv. + 126.

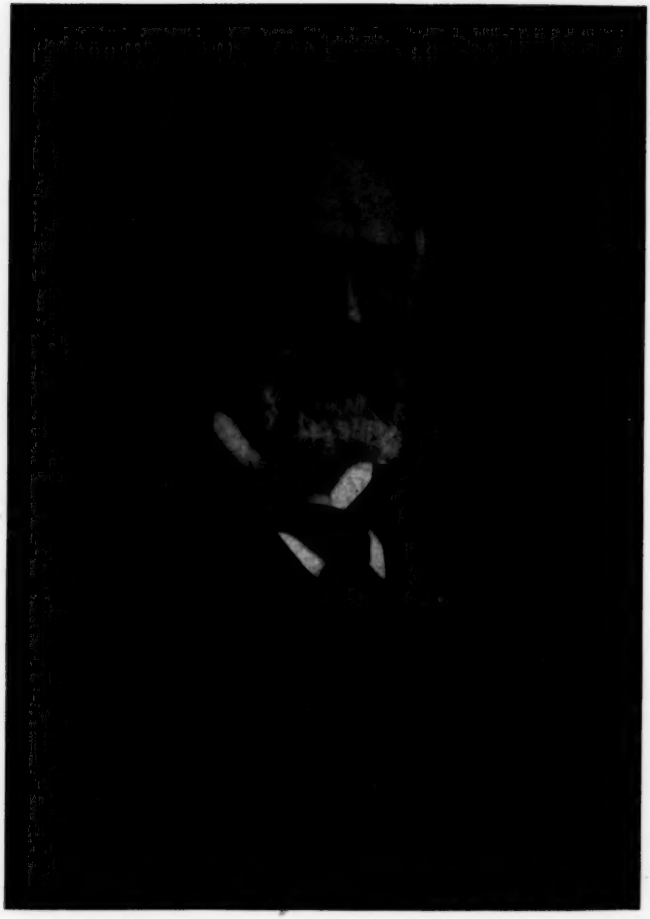
(3) H. Werner, Die Ursprünge der Metapher, pp. viii. + 238.

(4) B. Golz, Wandlungen literarischer Motive: i. Hebbels Agnes Bernauer; ii. Die Legenden von den Altvätern, pp. 94.

(5) H. Freyer, Die Bewertung der Wirtschaft im philosophischen Denken des 19. Jahrhunderts, pp. 174.

ARCHIV F. D. G. PSYCHOLOGIE

The *Archiv* passes, in its forty-sixth volume, from W. Engelmann to the Akademische Verlagsgesellschaft, Leipzig. The review-section will be resumed, and Professor Wirth hopes to publish four volumes yearly.



G. Stanley Hall.